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PSYCHOLOGICAL ATOMISM.

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We know the overwhelming richness of technical means through which the naturalist seeks the elements of his material. He dissects the organism into organs and the organs into tissues, and inspects the stained tissues under microscopes until he finds the cells. We know the complicated paths which lead the chemist to the complete analysis of every substance into measurable quantities of chemical elements and those which lead the physicist to divide the ray of light or the sound or the electrical discharge into elementary movements. The corresponding scheme of the psychologist must be very different. As his material is not accessible to the dissecting knife or to the dissolving chemical solution, he has merely the internal microscope of attention at his disposal. He dissolves the mental state into its distinguishable parts by turning the attention to the one or the other element, increasing the vividness of faint elements which were perhaps at first unnoticed and isolating the single parts of the complex by this inner accentuation. Such an isolation allows the linking of the part with all those characteristic associations by which we call a mental state known to us and recognized. The element of the idea which in the unity of the mental state was merely a vague unrecognized shade of the whole complex appears under the searching light of the attention as a well-known acquaintance, perhaps as a muscle or joint or pressure sensation or as the feeling of a special organ. All

technical outer appliances are but guides and helps for this attention. Resonators may help us to give our attention to the overtones contained in the sound, but we do not acknowledge as present what cannot be made accessible to the searchlight of attention. No outer analysis of physical processes can be substituted for this internal inquiry; we may know that the ray of white light contains all the rainbow colors, but so long as no appliance has been invented to enable our attention to discover other light sensations in our sensation of white it would be absurd to say that the white sensation contains the red and the green sensations.

This self-observational analysis meets no difficulty of a theoretical character in so far as the ideas, those of perception and memory and imagination and reasoning, are in question. If we call the distinguished parts of ideas sensations, the sensational structure of the ideas has become a problem which for a perfect solution needs much effort still, but theoretical obstacles seem not to stand in the way. With regard to the non-ideational mental states, the emotions and volitions, the situation is somewhat different. Analytical psychology has not been idle in this field, to be sure; but what has been offered as a result of the inquiry seems less satisfactory and less convincing, above all less final than the work on ideas. The general trend of this modern analysis of will and emotion can be understood from one point: they are described as combinations of sensations, that is as combinations of such elements as we find in our ideas. Every part of these emotional and volitional experiences, as soon as attention is directed to it, shows itself as an element which is known to us as a part of ideas also and is thus a sensation according to the definition. This does not mean that will and feeling themselves become ideas but that the elements of both appear to be the same. I said this result seems unsatisfactory: the attitudes of our personality seem to us in the immediate experience of our inner life given in a way so absolutely different from the passive presentation of ideas that we are reluctant to believe that they represent merely two different kinds of combination of the same material. Long before we think of analysis we feel that the ideas are given to us as

objects, while our emotional and volitional life is subjective activity. And yet it cannot be mere chance that every new attempt of scientific inquiry shows ever new sensations in that section of mental life.

The solution of this apparent contradiction can be seen only from a higher point of view than that of empirical psychology. The solution must come from an acknowledgment of the fact that mental life can be brought under two different aspects. From the standpoint of our immediate inner experience our will and feeling are never an object but an activity felt to be sharply separated from our ideas, which come to us as objects. But in that state of felt activity we do not have possible material for psychology. Psychology imitates the work of natural sciences just in that it analyzes and describes objects. Will and emotion are in reality incommensurable with ideas and their elements; in that the instinct of the hesitating sceptic is quite correct. But in that real stage will and emotions cannot be material for psychology at all and to make them such analyzable and describable things they must somehow be substituted by objects: in place of the original will and feeling we substitute the attitudes and actions and excitements of the perceivable organism and in so doing we deal indeed with objects whose elements are sensations; in that, the instinct of the modern psychologist is correct. Whenever the really psychological point of view is taken the emotions and volitions are just as truly objects, and as such just as truly content of consciousness as are the ideas.

At a first glance, to be sure, some one may say: psychology is indeed obliged to conceive will and emotion as objects, but that obligation does not imply the necessity that these objects have the same elements which are characteristic of the ideas. But even this objection is untenable. Description involves communication, but we cannot communicate a psychical object directly, as it lies in the nature of the psychical state to be exclusively the property of one individual: I cannot show my inner state to any one else and no one can give me a direct share in the contents of his consciousness; we can suggest to each other an understanding of our intentions and inner attitudes but every one must produce them in his own private consciousness. The

outer physical world is public property which any two subjects share with each other and about which a direct communication, a common taking hold, is possible. We can therefore communicate a mental state only indirectly and only in so far as it is necessarily linked with a part of the outer world. But this is true solely in regard to the ideas and their parts, the sensations. Every idea is necessarily related to the physical object which is referred to by the idea, and every sensation refers in the same way to a special feature of the meant object. This relation exists for no other groups of objects, or rather, wherever such a relation exists we speak of ideas. To make psychological objects describable means, therefore, to conceive them as complexes of possible elements of ideas; and as the presupposition must be that the psychical world is finally describable and thus accessible to science, we can say *a priori* that wherever an inner state is felt which cannot be fully described to-day in terms of sensations there is to be found a problem for to-morrow; but it is settled beforehand that whatever the result may be it must be a result in the language of sensations. To say there is an indescribable feeling or impulse is never to announce a psychological result, but only an unsolved psychological problem. The total mental life is thus for psychology a complex combination of coördinated sensations, and that statement can be accepted even when science is still far from the point where all these sensations shall be identified and recognized. These sensational elements are different from each other in various respects. They show, first, a qualitative difference which may be as small as that between the pitch of two neighboring tones or nuances of color, or as large as that between tone and temperature sensation or color and smell sensation. They show, secondly, differences of intensity, as every quality may vary from the weakest impression to the most intense; and, thirdly, each sensation may be given in any degree of vividness. The original character of this last category cannot be denied and cannot be thrown together with intensity, as the weak sensation may be vivid and the strong one quite unvivid in our minds.

We stand now before the chief question of this inquiry: Are these sensations the ultimate elements of the contents of our

consciousness, or is that which we call a blue or hot sensation, a sweet taste, a tone C, a muscle sensation or a pain sensation itself a complex affair which consists of more elementary parts: in short, have we in the mind ultimate elements which are simpler than the sensations? It is the inquiry for a radical psychological atomism, and even those who would reject the proposed view that all psychological facts are complexes of sensations must acknowledge the right and importance of this question. Only the bearing of this question is for them not the same as for us, because for them it means only the inquiry about the ultimate elements of the ideas, while for us it means the question about the ultimate elements of the whole psychological life. It seems at first surprising that psychology in its modern form has hardly ever seriously raised this question, and has always stopped in its analysis as soon as the distinguishable sensations have been reached. Physics did quite otherwise: it never stopped at the point where the observation of the biologist or physicist or chemist found the last mechanically separable parts. Theoretical physics went far beyond that point, and saw its goal in a description of the physical universe according to which those cells and molecules and chemical substances are combinations of atoms which are unperceivable; and the atomistic theory of the universe which necessarily transcends empirical observation is to-day the basis of all natural science. Why has psychology never felt this demand which seems to physics so profound and so natural? I must here refer for a moment to philosophic arguments, as all these functions of the scientist, carried out by him instinctively and without philosophical motives, must have had their deeper reasons. We must ask whether the difference in the nature of his material suggested this modesty in the analyzing psychologist, while the physicist rushed without fear into a most radical atomism.

What is the ultimate meaning of all description? The empirical scientist may say that by the communication of the elements he gives an understanding of the object as it really is. But can we accept this from a higher point of view? The naturalist dissolves the objects into parts, but those heaps of separated parts are no longer the original object which he

started to describe. We may show that we can analyze a house into bricks and boards, a plant into cells, a chemical substance into several elements, but we have no right to say that those cells are a plant, those bricks a house, those elements the complex substance. What we really mean is only that we can transform the one object into those other objects. I can transform the house into single bricks and the plant into single cells, but I cannot transform the plant into bricks and the house into cells; that is what the description of the elements means. The enumeration of the elements does not approach the reality of the object nearer than the mere perception of the object as a whole does, but it allows us to judge of the possible transformations of the object; we learn what we have to expect from the object in the future, we understand the effects which the object must produce; that is, our descriptive enumeration of elements is intended to give us a causal understanding of the changes which go on. In other words, there is no description which is not a preparation for causal explanation, and what we call elements are only the foothold for our expectation of possible transformation. Description and explanation are inseparable. All progress of causal understanding means a progress in the full description of objects, and the descriptive search for elements has no scientific right to existence if it does not prepare a new understanding of causal connections. The claim of the naturalist that the world as physical object is made up of physical atoms is thus merely a special expression of the demands of physical explanation. Not a desire for an exact description for its own sake, but the desire for a consistent causal interpretation of the physical universe has brought about the atomism; the world must be conceived as a system of atoms to allow those expectations in regard to causal changes which modern physics demands.

The situation in psychology is in principle the same. We have said that there all analysis is based on the isolation which comes as a function of attention. But if our attention separates the different parts of an idea, we can say again that such a series of sensations is no longer the original idea; that group of separated sensations is a new content of consciousness, and if we

say that the idea contains those sensations as elements we really mean that by an action of attention we can transform the idea into that series of sensations. The passing over of the idea into a series of isolated sensations is the only fact which we seek to express by the statement that the sensations are parts of the idea. The idea of a painting can be transformed into a series of visual sensations, the idea of a melody into a series of tone sensations, while our attention can never expect to transform the melody into a series of visual sensations. Description means thus, in psychology exactly as in physics, the enumeration of those characteristics of the object which determine our expectations as to the transformations of the object. Ultimately then not the isolated object but its connections, its transformation into something else, is the material of the description. The analytic interest of psychology therefore can continue only so long as the discovery of elements means an understanding of the connection of successive mental states. Has psychology in that respect the same interest as physics in carrying out its analysis to the last possible elements?

The situation of psychology is here decidedly different from that of physics. All connection between physical objects is a direct one; one movement is the directly necessary cause of another movement. On the other hand no psychical process conceived as an object of consciousness is ever in itself the direct cause of another psychical process. No direct causality ever links two psychological contents. We may formulate generalizations of observed successions, but they never bring us that causal necessity which mechanics knows. Or we may think of the inner connections of our subjective attitudes, the inner connections to which our freedom and unity and ethical life belong, but then we are no longer dealing with psychological objects. As soon as we want causal connections between these mental objects we must seek an indirect connection instead of the direct one, that is, we must link the mental states with physical objects. Even the passing over of the idea into a set of sensations under the light of the attention is not a change which has a necessary character; we can never understand why the one must bring the other. It would lead us too

far here to ask why that is so, why physical objects can be connected directly, psychical ones indirectly only. The reasons lie in the nature of the object; the physical object, since it is an object for all, lasts and can never disappear, the psychical object, since it is an object only for the one subjective act, can never return; the new psychical object can only be like the old one, it can never be conceived as the same. All causality is based ultimately on identity, and in a world of objects which cannot have a material lasting from one act to the next no direct causality is possible; for the purpose of a causal connection it must be conceived as linked with the lasting material of the physical world. But as soon as we understand that all psychical explanation is dependent upon the connection of the psychical facts with the physical ones, it is clear to us that our conception of this psychophysical relation must determine the point to which the psychological analysis shall progress. An analysis which is not in the service of explanation is, as we saw, useless and meaningless. If we see now that explanation in psychology must always be indirect, based ultimately on the causal connection of physical processes, it is clear that psychology has no reason to investigate elements which have merely a psychological interest and no bearing on the psychophysiological explanation.

It was therefore not by chance but from an instinctive feeling of the general philosophical situation, that psychologists have so far given hardly any attention to the question whether we can push analysis beyond the limit of the sensations, simply because sensations alone have appeared as those results of psychical analysis which can be linked with simple physical processes. Sensation is not, as is usually said, the simplest element which psychological analysis can find, but it is that factor of the mental states whose corresponding physical stimulus in the outer world cannot be dissolved any further without ending the mental effect. We cannot say that the physical process which corresponds to the sensation is in itself elementary from a physical point of view; not at all. But any part of the stimulating process into which atomistic physics may decompose the stimulus would no longer correspond to a distinguishable part of the psychical state. The sensation is thus not the simplest mental

state but that simplest mental state which can be used for a psychophysical connection, as the further dissolution of its physical correspondent would no longer go parallel to the psychological analysis, and any pushing of analysis beyond the sensation would thus be useless for explanation and therefore theoretically unjustifiable.

This was the philosophical background of psychological analysis in the past. But have we a right to stop here? Is the physical correspondent of the sensation really the simplest process which can serve as a basis for psychological explanation? I do not think so. If our psychophysics were based on the connection between the sensation and its physical stimulus outside of the body, then certainly we could not go any further. If the sensation appeared in consciousness only when that stimulation outside of the body went on, then it would be absurd to divide the process still further, as we have acknowledged that a division of the physical stimulus means the end of the sensation. But we all know that modern psychology has worked out a further development of the system of psychophysical connection, according to which we substitute for the stimulus that physiological brain process which is causally produced by the stimulus. Thus as basis for the psychological explanation we link color, not with the ray of light, but with the process which such a ray of light produces in the occipital center of the brain. Has the situation not essentially changed by this physiological appendix to the psychophysical theory? The outer stimulus which corresponded to the sensation cannot be divided further without breaking up the sensation; but is that true for the physiological brain process which is produced by the stimulus and which is now the process corresponding to the sensation? We have been too long satisfied with the hypothesis that the elementary stimulus of the outer world excites merely the physiological unit in the brain which is represented by the isolated ganglion cell. Physiological psychology has had at no time the right to claim that the anatomical connections or the physiological experiments have demanded or even supported just this hypothesis. It was simply an expression of lack of knowledge. We cannot say that more recent inquiries have put clear facts in the place of hypothetical ideas,

and yet it may be claimed that everything points toward a much more complex idea of the physiological substratum of the sensation. We are becoming more and more accustomed to the view that the most elementary stimulus brings about a nervous excitement which grows from the periphery to the center like an avalanche and stimulates in the brain a whole area which may overlap the area stimulated by another isolated excitement. If thus the simplest idea is the accompaniment of a process in a multitude of cells, the question arises at once, Which psychical state corresponds to the process in a single cell? To say that each cell of the set brings about that same sensation which is observed as a result of the multiplication of the process would be utterly useless; the idea that the psychical process which corresponds to the excitement of the physiological unit is something simpler than a sensation is thus natural from the beginning. As soon, therefore, as it is recognized that the brain-effect of the indivisible stimulus is an easily divisible physiological process the sensation ceases to be the ultimate result of psychological analysis, since a still more elementary psychophysical unity can now be conceived as the last factor in the explanation.

Under these circumstances it becomes the duty of the psychologist to prepare the way and to ask how far the psychological facts in themselves suggest such a further analysis. I enter reluctantly into this new field, and yet I am convinced that the time has come when we need a psychological atomism just as much as an atomism of the physical universe. The ultimate psychical elements will be like the physical atoms in that they are merely constructions for the purpose of explanation, not perceivable objects; just as no naturalist has ever seen or touched an atom, no psychologist will ever observe a psychical atom in his consciousness. That must be the case, because every distinguishable part of the idea must correspond to a special characteristic of the object of the idea, and as long as this ideational correspondence holds true we call the element a sensation; a part of the sensation must be thus unperceivable and yet it may be clearly determinable and it is certainly in consciousness just as the unperceivable atom is in space; it remains unperceivable as it is indistinguishable in that complex unity

which we call sensation, while every sensation may be related to the psychical atom as the symphony to a single tone.

The psychological fact which stands immediately in the foreground of such considerations is the fact of the similarity of the sensations. Every sensation finds its place in the empirical system by its similarity with others; we bring all light sensations together in one group because each has with every other more similarity than with any sound or smell sensation; among the light sensations each again finds its place among those most similar to it, each color being placed next to the colors which stand nearest to it in the spectrum and also next to the least differing intensities of the same color tone. Thus there is known to us no sensation whose exact place in the manifoldness of mental life is not determined through its similarity with others. What is this similarity? At first glance it may seem sufficient to say that two objects are the more similar the more easily they can be mistaken for each other; and as this would be possible for absolutely simple objects the similarity of the sensations would not militate against their simplicity. But this possibility of confusion does not imply anything as to the content of the two objects; it refers merely to their position in the psychical connections. That two ideas can easily be confounded with each other means that both under special circumstances have an equal influence on the stream of mental facts. To report the influences of a mental object is no description of its structure; as soon as we begin to describe the content of similar objects we have a foothold only in the analysis of complex objects. There we call two ideas similar whenever their component parts are partially identical. Similarity from the point of view of description is community of parts; these common elements of our ideas may be outer sensations or sensations of bodily origin in the case of similar form or rhythm, and so on. The logical conclusion by analogy is that two sensations also are similar to each other only when they contain various component parts of which some are common to both. These parts are of course not sensations but inexperienceable factors like the atoms, and as we do not know a sensation which is not in some way similar to some other one, we can say that no known sensation is an ultimate element.

Just as the atoms of physics are conceived under the general conditions of all-space filling objects, so the psychical atoms must be conceived under the general conditions of consciousness. Therefore as no conscious objects can be twice together in consciousness, it may be said at once that these ultimate elements do exist or do not exist in a state of consciousness but that they cannot exist more than once. Thus even these elements cannot be used for the purpose of constructing the strong sensations from a large number of equal atoms of which perhaps a smaller number constitutes the weak sensations. No; ever new qualities must come together to form a new sensation, it may be a new nuance or a new intensity; in the stronger sensation must be a new quality which was absent in the weaker sensation. It seems inadvisable, however, to apply this treatment to the variations of vividness; a sensation has similarity with another of different quality or different intensity; but it has no meaning to say that it has similarity with the same sensation of a different degree of vividness. We must thus acknowledge that the change of vividness is a category for itself which cannot be eliminated by a reduction to varying combinations of atoms.

So far, then, the only description which we can give of our undivisible elements of psychical material is that they are absolutely dissimilar to each other; as long as two are conceived with the slightest similarity to each other we must acknowledge that the analysis was not really ultimate. Each of these absolutely dissimilar elements can pass through all degrees of vividness down to the point of disappearance. The interesting relations which exist between quality, intensity and vividness suggest, by the way, from the beginning, that the variations in vividness of the atoms may bring about not only changes in the vividness of the whole complex which we know as sensation, but also changes in quality or intensity. We could think, for instance, that the vividness of the sensation changes as soon as the vividness of all its component atoms changes equally, while the intensity or quality of the sensation changes as soon as the relation between the degrees in vividness of the various elements varies.

The study of the similarity of sensations thus allows us to

determine conceptionally the elements which we must postulate, but we do not know from it anything about their mutual relations. Only one thing is evident—that all the demarcation lines which existed for the sensations have now disappeared. The sensations were grouped just by their similarity; the psychical atoms, which have no similarity, are thus without the benefit of this parceling. But their very disappearance prepares the way for a deeper understanding of the relations between the sensations, as the similarity of sensations of different senses then offers no difficulty; the similarity between smell and taste, or between touch and muscle sensation, and so on, appears then not different from the similarity of two tones.

Where shall we find a foothold for the construction of the relations which exist between our postulated elements? Again we shall of course be guided by the analogy with observable processes of the complex states. An idea is related to other ideas by associations and inhibition; it awakes other ideas or suppresses other ideas. We have no reason to believe that this power of mental contents is dependent upon their complexity; we can thus demand the corresponding possibility for our elements; they may awaken other elements or may inhibit them. What can we know about the special facts of this kind in our atomistic miniature psychology? With regard to association, of course, we can claim that every real sensation shows us a case of such association of atoms; thus the analysis which we attempt on the basis of similarity merely expresses in a disintegrative form that which our atomistic association laws would express in a synthetic form. We have the results of associations in every experience of sensations before us; we do not need anything further in that respect. Quite differently with the indispensable correlate, inhibition. How can we ever find out anything about the inhibitory powers of our elements? The one strong light which seems to fall on this difficult problem appears to me to come from the fact of fusion of sensation, a fact which has itself only in recent years attracted the attention of specialists. We consider two mental contents as fusing when their simultaneous appearance renders their separation difficult. Even the unmusical person hears two tones when two

tones in the interval of a seventh are given; but it happens that he sometimes takes them for one tone when they are in the interval of the fourth, and more frequently with the fifth, and almost always with the octave; that is, the octave fuses most, the fifth less, the fourth less, the seventh still less than that, and so on.

The fusion of tone sensations is the easiest to study, but other sensations show the same relations. Every one knows how strongly smell and taste sensations fuse, or touch, muscle- and joint sensations, or color sensations with colorless light sensations, or touch and temperature sensations, and so on. Of course if we say that fusion is characterized by the difficulty of recognizing the two fusing ideas or sensations we do not give a real analysis of the processes but speak merely of its mental effects on our associated judgments. But we can perhaps try to analyze the process itself. In every fusion each of the fusing parts becomes less recognizable because each part is deprived of something of its own character; with respect to its atomistic aspect we say that each of the component parts loses some of its elements. The sensation *A* may contain the atoms *abcd*, while sensation *M* contains the atoms *mno**p*. We say that *A* and *M* do not fuse if *abcd* and *mno**p* can coexist, while we have a small degree of fusion as soon as *A* suppresses, for instance, the *p* in *B* and the *M* suppresses the *d* in *A*; the result in the following together of *A* and *M* is then *abcmno*; and if merely *abmn* remain the fusion is more perfect. In this way fusion transforms itself into the inhibition of psychical atoms and allows a deep insight into the working mechanism of the ultimate elements. But from this point of view the inhibition of the elements means nothing but the decrease and association the increase of their vividness, so that this group of determinations also is reducible to the conception of vividness.

We thus have for the atomistic constitution of our psychical world the following suggestions: First, we know that they are elements which, while under the general conditions of consciousness, are different from our sensations; they are absolutely dissimilar to each other; they can vary through all degrees of vividness; but they are all coördinated, not belonging to any

special groups according to our special senses, and they are the material for building up in their qualitative manifoldness both the so-called qualities and the intensities of the sensations. Their existence must be worked out from the similarity of the given sensations. Secondly, these atoms can pass not only through all degrees of vividness themselves, but they can influence the vividness of other atoms. They may increase the vividness of other elements, that is, produce associations; the nature of these associated groups of atoms we can work out from the given sensations which represent all the known groups of such associations. On the other hand, they may decrease the vividness of other elements, and the facts concerning this suppressing influence we can work out from the empirical facts of that partial inhibition of sensations which we call fusion. The similarity of sensations thus shows us the way to the elements themselves, while the variety and the fusion of sensations show us the way to the mutual influence of the elements. All these influences are reinforcements and suppressions of vividness, and thus we can reach from the empirical study of the sensations, especially with regard to their similarity and fusion, a system of psychical atoms which are absolutely dissimilar to each other, but which stand in a mutual relation of increase and decrease of vividness. The different degrees of this mutual influence are then, of course, a natural basis for a systematic order of the manifoldness which cannot be ordered according to similarity.

This new world of atoms in psychology—that is, of really indivisible elements of mental facts—thus answers the same purpose which the physical atoms answer, but on account of the unlikeness of the material their character is different in every respect. The physical atoms are all qualitatively alike, that is similar in the highest possible degree, while the psychical atoms are absolutely dissimilar. The equal physical atoms get their individuality from their position in space; two atoms are two, not because they are different, but because they are in different places; in psychology two atoms are two, because they have qualities that cannot be compared. In the physical world all the processes of the universe are to be expressed in one function of the atoms, namely, movement, a change of place with change of

time; in psychology all processes of the psychical world are to be expressed in the one function, change of vividness. All the energies by which the physicist explains the mutual influences of bodies are functions of movement, and all the associations and inhibitions and fusions by which the psychologist explains the mental life are functions of vividness. The system of qualitatively similar elements in space which affect each other merely by influence on their movements, and on the other side the system of absolutely dissimilar elements in consciousness which affect each other by influence merely on their vividness, are thus two logically equivalent constructions which have equally the character of an ultimate systematization of the two worlds of objects.

Of course, we must not forget that on the psychical side the word 'influence' has not the same meaning as on the physical side; it has no real explaining power, as we have seen, till the psychical relation is linked with a physical one. But at this point everything seems ready for an adjustment. As soon as we conceive such an ultimate psychical element as the accompaniment of a single ganglion cell we understand without difficulty its associative and inhibitory influence on other ultimate elements. We must simply get rid of that unbased idea that a sensation has one physiological unit as its basis. In the beginning of physiological psychology it was quite usual to think that the whole idea of an object corresponded to one cell. At present this is corrected: we know that each idea contains many sensations and we give a cell over to one sensation. In the same way we must understand that every sensation is a very complex system and that only its elements, the psychical atoms, can rightly be correlated with the physiological units. All the difficulties in the way of the physiological foundation for the similarity and fusion of sensations, and so on, will then disappear and we shall have a really consistent system of psychophysics.

Only the one point must always be kept in mind that even these ultimate psychological elements can never be ultimate metaphysical realities. We have seen that psychical atoms, just like physical atoms, are constructed for the purpose of understanding the connections in the two systems of objects

which we perceive, but the one principle which we recognized from the beginning was that the aspect from which mental life appears at all as a psychological object is not the aspect of reality and life. In our life the inner experience is a felt system of will relations and a metaphysically ultimate factor must thus always lie in this immediate reality which we have left behind us when we conceive the world as physical and psychical objects. The psychical atoms are thus not more true as philosophical realities than any other parts—as, for instance, the sensations, or the molecules—both are merely constructions for logical purposes; but with these purposes constantly in mind, we must begin to see that the sensations are unsatisfactory as ultimate conceptions, and that they must be replaced by psychical atoms if psychology really desires to become worthy of its great older sisters, the natural sciences.

SHADOW IMAGES ON THE RETINA.

BY F. H. VERHOEFF, PH.B., M.D.

[From the Physiological Laboratory of Johns Hopkins University.]

In connection with the shadows thrown upon the retina by a small object, a pin for example, placed within the focal distance; I have recently noticed a striking optical phenomenon that is the exact opposite to the well-known pin-hole shadow experiment. The usual pin-hole experiment is produced by pricking a small hole in a card which is then held before the eye, but within the point of distinct vision. Under these conditions, if a pin is held between the hole, which serves as a source of light, and the eye, it casts an erect shadow upon the retina, and this shadow is projected as an inverted image of the pin.

This experiment was described and explained by Le Cat¹ in 1740. It is possible, however, that he was not the first to describe it, for he does not, in his work, claim that the experiment was original with him. The following is a translation of his description (of Plate I., Figs. 1 and 2) of the phenomenon:² "Without betaking yourself to a dark room, put before your eye *D* (Fig. 2), a black card *B*, pierced by a pin-hole, *C*; place opposite to and beyond this hole a very light body, such as a sheet of white paper, *E*, illuminated by a candle, *G*; place now a pin, *d*, before your eye, *D*; you will see with surprise the pin inverted, and on the other side of the hole, *F*. Let us see how this reversal and this transposition comes about. You understand that the images of external objects in passing through a hole (Fig. 1) are inverted and are depicted thus inverted * * * in the eye, *D*; it is the same with the images which pass through the single pin-hole,

¹ Le Cat, *Traité des sens*, Rouen, 1740, p. 297 (Helmholtz).

² Le Cat, *Traité des sens*, Paris, 1767, p. 507. I have not been able to obtain the earlier edition referred to by Helmholtz.

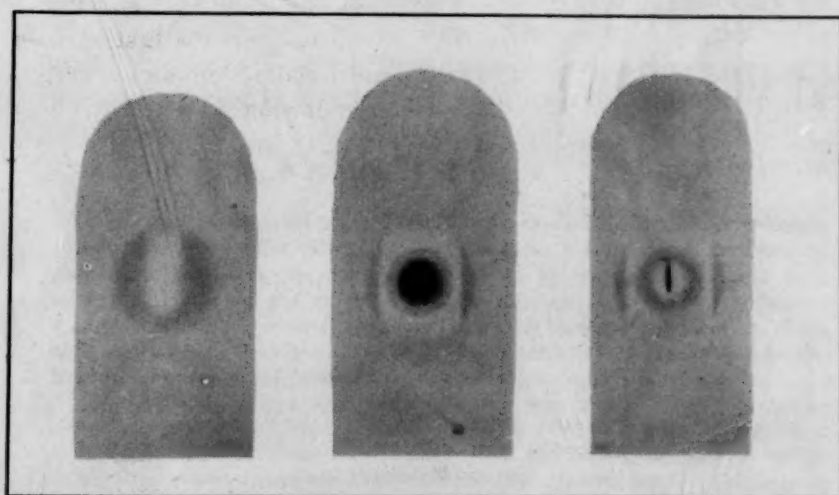
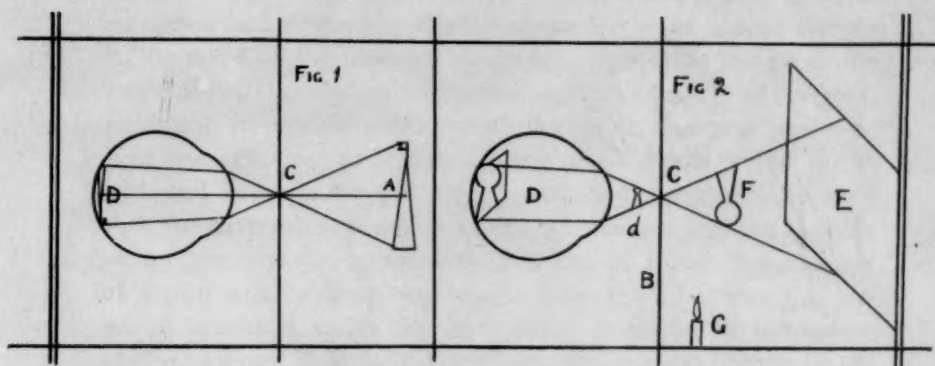


FIG. 6.

FIG. 8.

FIG. 9.

Verhoeff on Shadow Images on the Retina.

C (Fig. 2), and which are depicted in the eye, *D*. At the spot where the upright pin, *d*, is placed, the images are already inverted; now this pin meeting these inverted images arrests the rays which correspond to it and produces consequently in these images a lack of rays, a shadow of the figure of a pin; the pin in the midst of the inverted image is upright; the image of the paper, *E*, will then be depicted at the fundus of the eye as inverted, having in its middle a shadow of the pin in a correct position; now one perceives as upright objects which are inverted in the eye and reverses those which are upright, there will be seen then the external object *E* in an upright position and the shadow of the pin inverted; moreover, this pin, or rather this shadow of a pin, will be seen beyond the hole at *F*, because the pin which is seen is really but the shadow produced in the image of the external object *E*; this phantom pin ought then to be referred to the external object *E*, and be seen beyond the hole."¹

Le Cat is entirely correct in his explanation so far as he goes, but he fails to mention the importance of the position of the pin-hole with respect to the conjugate focal point of the eye, and he also fails to explain fully why it is necessary for the pin-hole to be of small size. The mention of the white sheet of paper, *E*, in the explanation is unnecessary and also misleading, since it distracts attention from the essential points involved.

J. L. Tupper, in a letter to Professor Tyndall, published in

¹ Scheiner (*Oculus*, 1659, p. 49) described an experiment similar, in principle at least, to that of Le Cat. The following description of it is taken from 'Priestley on Vision,' Vol. I., p. 114, 1772: "If an object be suspended in a small hole made in a thin board, and an eye, situated in the dark, look through the hole at a number of torches or other luminous bodies, he observes that as many small objects will be seen as there are torches, etc. In fact, it is the shadow of the object made by each of the luminous bodies that is cast upon the eye."

In the diagram given by Scheiner the hole was represented as a comparatively large one—much larger than the pupil—and hence it could have nothing to do with the experiment. It is essential that the torches should be out of focus, a fact evidently unknown to Scheiner. The torches act as small sources of light, corresponding to the pin-holes in La Cat's experiment, but since they are beyond the conjugate focal points of the eyes, the shadows of the objects that they throw upon the retina are inverted, and hence appear erect. This experiment should not be confused with the experiment of Scheiner referred to further on in this article.

the *Philosophical Magazine*¹ for 1870, gave what he thought was the first correct explanation of the phenomenon. He was, however, evidently unacquainted with Le Cat's work on the subject. Tupper's letter was called forth by an entirely erroneous explanation given by Michael in No. 251 of the 'English Mechanic.' Tupper states the essential explanation in a few words when he says of the experiment, 'it negatives delineation by cones and admits of it by single rays only.' This is not, of course, strictly true, since it would be impossible to obtain a hole through which only single rays could pass and still give enough illumination to make the shadow evident, but the more nearly this condition of affairs is approached the sharper is the shadow. For this reason, therefore, it is necessary that the pin-hole should be small.

Le Conte,² in 1871, published a short article in which he takes exception to Tupper's calling the impression of the pin on the retina an image, Le Conte insisting that it is a shadow. He says: "Now, there is no doubt that this explanation is substantially correct; but it would have been much clearer if Mr. Tupper had distinctly expressed the fact that the retinal impression in his experiment is not an image, as in ordinary vision, but a shadow. Mr. Tupper seems to have perceived the distinction but has not kept it in his mind, and hence some confusion in his deductions."

Tupper thought the experiment disproved the law of visible direction and gives the following in support of his view: "A second deduction regards the law of visible direction by which it is said an object is seen in the direction of a perpendicular to the retina at the object's point of incidence; and it is cited by its promulgator to prove the sense of vision primary and unacquired; therefore, its operation should be absolutely efficient and unindebted to experience, unconditionally indicating the direction of the object. But the law proves fallacious, the object not being in the direction of the perpendicular; it is seen where it is not. That it is seen under novel and inexperienced conditions cannot be urged without admitting experience as a

¹ Vol. 39, Fourth Series, p. 423

² Joseph Le Conte, *Philosophical Magazine*, London, Vol. 41, p. 266.

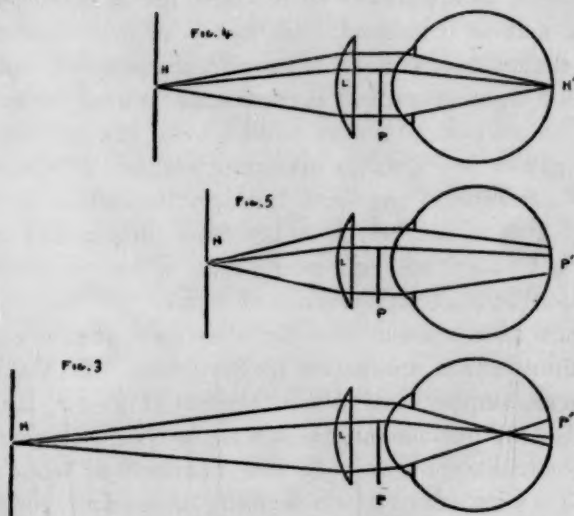
prerequisite to sense of direction." On this point Le Conte says: "The experiment confirms the law, as indeed does every phenomenon of vision. The law of visible direction is that every point of the retinal impression is referred to the field of vision along a line passing through that point and through the optic center. . . . This gives the pin inverted as we actually find it."

Le Conte was of course undoubtedly right in his statement. Tupper evidently did not have a very clear idea of what the law of visible direction is. It simply relates to the direction along which an object is apparently seen, not to the actual direction of an object, and is consistent with either theory of vision, the acquired or the congenital. The phenomenon not only confirms, as Le Conte says, but it proves the law of visible direction for it shows that no matter what may be the position of any retinal image or shadow, its apparent position is indicated by lines drawn from the image and through the optic center of the eye. As a rule of course visible direction corresponds to actual direction, but this is not necessarily true where the objects are smaller than the pupil and are out of focus.

Tupper makes another mistake when he says that a pin seen *through* a pin-hole is uninverted on the retina. Le Cat showed that this was not the case in his diagram (Fig. 1). Le Conte also rightly says that vision through a pin-hole is in all respects similar to ordinary vision. In this connection, however, Le Conte makes a statement which is really misleading, for he says, comparing the retinal impression in pin-hole vision to that in the experiment under discussion, "the one is a *true image*, the other a *shadow*; the one is *inverted*, the other *erect*." If Le Conte meant to imply by this that the reason the latter should be called a shadow is because it is erect, he was in error, for I shall show that it depends altogether upon the position of the pin-hole with regard to the conjugate focal point of the eye as to whether the impression is inverted or even as to whether the impression occurs at all. However, the term shadow is very appropriate, although I think shadow image would be still better. Le Cat, Tupper and Le Conte seem to have entirely overlooked in their respective explanations the importance of the position of the

pin-hole. Le Conte in his work 'Sight'¹ claims that he first explained the phenomenon and refers as proof to the article just mentioned. As a matter of fact he added little to what Le Cat and Tupper had already said.

To show the dependence of the position of the projected image upon the relative position of the pin-hole, the experiment may be modified in the following interesting way. If for instance the eye is focused for the near point and the pin-hole is held beyond this point, the shadow of an interposed pin will appear erect. Under these conditions if the pin-hole is grad-



ually brought closer to the eye, the image of the pin becomes more and more blurred, and finally disappears when the hole is at the near point, that is, when the eye is accommodated for the source of light. If the hole is brought still nearer, the original experiment is reproduced, the image of the pin again appears: but is now inverted.

¹ Joseph Le Conte, *Sight*, p. 87, New York, 1881. In the edition of 1897, p. 74, Le Conte says: "This phenomenon was explained by the author in 1871. See *Philosophical Magazine*, Vol. LXI., p. 266. It had, however, been previously explained by Priestley, but forgotten (*Nature*, Vol. XXIV., p. 80, 1881)."

I find that Priestley (*Priestley on Vision*, Vol. II., p. 725, London, 1772), briefly summarizes Le Cat's description of the phenomenon, referring to '*Traité des Sens*,' edition of 1744, and gives all credit to Le Cat.

This experiment is rendered easier if the accommodation is assisted by a convex lens, one of about ten diopters being very convenient. The lens is held close to the eye and the pin in front of the lens, or better immediately behind it. Then if the pin-hole be placed beyond 10 cm. from the supposedly emmetropic eye, it will appear out of focus and the shadow will be upright. As it is brought closer, the phenomenon just described takes place. The accompanying figures (Figs. 3, 4, 5) explain this experiment. In each figure, *H* is the pin-hole, *L* the lens, *P* the pin. In Fig. 3, *H* is beyond the focal distance of the lens and the shadow appears erect. In Fig. 4, *H* is in focus at *H'* and no shadow is formed. In Fig. 5, *H* is within the focal point and the shadow appears inverted. In place of a pin-hole, a small white spot on a black surface may be used as the source of light.

I shall now describe the phenomenon referred to at the beginning of this article. If a sheet of white paper or a piece of ground glass is taken, and a small black spot is made upon it about the size of a pin-head, and this spot is used in place of the pin-hole in the above experiment, a white streak will be seen crossing the black spot (Plate I., Fig. 6). The phenomenon is best obtained without a lens, and is not an easy thing for most persons to see, since it requires that the accommodation shall be relaxed at will. I shall for convenience speak of this light streak seen crossing the black spot, as a white shadow. This white shadow behaves in exactly the same manner as does the black shadow in the pin-hole experiment—under the same conditions it may be seen upright, inverted, or be made to disappear. The experiment should be conducted in a good light, and a piece of white paper not less than five centimeters square should be used. This phenomenon may also be obtained with a photographic camera.

The explanation of the phenomenon is not quite so simple as in the case of the black shadow. The explanation suggested is illustrated in Fig. 7. This figure represents an eye accommodated for parallel rays; *AB* is the diameter of the black spot and *PP'* is the cross section of the pin. The exact relative sizes of spot and pin are not represented, because the lines would

then have to be drawn so closely together that they would not be seen clearly. Following the usual optical constructions, it will be seen that the black spot causes an area of diminished brightness, represented in section as $B'A'$. The lines drawn from AB denote the rays which would have come from it had it the same luminosity as the rest of the visual field; the lines BC and BD represent the limits of the rays stopped by B , and the lines AC and AD represent the limits of the rays stopped by A . BC would strike the retina at B' and AD would strike it at A' . The continuations of AC and BD are not drawn, but would fall between B' and A' , as would all the rays from points between A and B . Hence $B'A'$ would evidently have less illumination than the rest of the field, but would still be illuminated to a certain extent, since all rays parallel to the primary axis of the eye would fall upon its center, and the rest of it would be illuminated by rays parallel to the secondary axes which strike it, of course, at every point of its surface. Now if the pin PP' is put in front of the eye, there are still more rays cut out of the general illumination, PCI and $P'DN$ denoting their limits. The pin will therefore cause a diminished brightness, extending from I to N , and this is seen to include $B'A'$. But the rays included between APT and $BP'F$ have already been cut out of the field by AB , and therefore FT will have

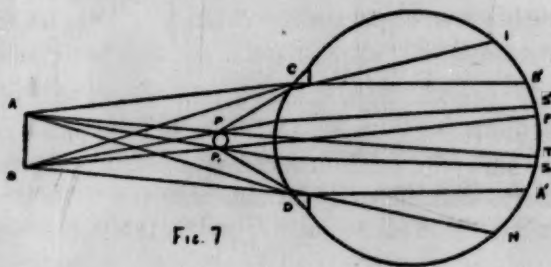


FIG. 7

the same illumination as before, and since $B'S$ and $A'S$ have less illumination than before the pin was placed in position, FT will, by comparison, appear as a light band cutting the darkened area $B'A'$. In the figure, $B'A'$ and FT represent, of course, only cross sections of areas. A figure representing

the pin in longitudinal section may be drawn by a construction similar to Fig. 7.

The white shadow is not absolutely clear cut because the edges shade off, owing to the fact that the original illumination of FS and TS is gradually diminished from F and T outward until the maximum diminution in brightness is obtained extending from S' to B' and from S to A' . Since the length of the pin is greater than the diameter of the spot, a certain number of rays which would have fallen on FT are cut off and there is an actual *slight* diminution in the illumination of FT .

That physiological contrast plays a part in the perception of the white shadow is indicated by the fact that the area it occupies seems brighter than it did before the pin was brought across it, and is further shown by the fact that a red spot will give a faintly blue or green shadow. In this case, $B'A'$ (Fig. 7) would have an area of mixed red and white light, with the exception of FT , which is really white since the red rays are prevented from reaching it by the pin.

It is obvious that one of the most important factors in the formation of the shadow is the amount of illumination of $B'A'$, for it is the decrease in the illumination of the outer parts of $B'A'$ that makes the white shadow perceptible, and if the illumination is already very slight, even a total lack of light in certain parts would not be noticed. It is hardly necessary to state that the illumination must not come from the spot—the blacker the spot the better. If the black spot were in focus its image on the retina would be entirely black and the pin could not, for this reason alone, have any influence upon it, except perhaps to make the whole spot look lighter since it would diminish the brightness of the rest of the field.

If, in the place of a small spot, a sufficiently large one is used, it will be noticed that on relaxing the accommodation the center (of its image) appears much darker than the rest of the image. This is due to the fact that as the spot is made larger it cuts off more and more rays which would otherwise have reached the center of its image, and when it is as large as the pupil it cuts off all the rays parallel to the primary axis, thus producing a small spot in the center of its image totally devoid

of light. But even when the spot is much less than half the size of the pupil the image appears darker in the center, since many rays will even then be cut off from the center of the image. This circumstance explains the fact that if a moderately large black spot is used for the shadow experiment, the white shadow of the pin is broken by a dark central spot into two white lines, somewhat resembling crescents, and giving the appearance represented in Fig. 8. If a large black spot with a small white one in the center is used, the appearance of these two crescents may be obtained, and in addition the usual black shadow is seen over the central white spot (Fig. 9).

As to the diameter of the pin which can be used the theoretical limits are from a geometrical point to a diameter equal to the distance between the lines AC and BD , measured at the position the pin is to occupy. These do not indicate practical limits, however, for a very small pin does not cut off enough light to make sufficient difference in illumination on the retina, and in case of a large pin too much outside illumination is cut off to make the phenomenon apparent. As the diameter of the pin is increased it cuts off more and more rays from the center of $A'B'$, and by the use of a moderately large pin and black spot together with a convex lens I have been able to obtain a very faint, ill-defined dark line in the middle of the white shadow. It is possible, however, that this may in part be due to contrast. The size of the shadow for a fixed size and position of pin varies with the size of the spot. The distance the spot is held from the eye changes inversely as the actual diameter of both the image of the spot and the shadow, but the relative proportion of shadow to image remains constant. Other things being constant the size of the shadow varies with the distance of the pin from the eye, but not in a simple relation. The size of the shadow varies also with the size of the pin, but no matter how small the diameter of the pin its shadow can be no smaller than the diameter of the image of the spot when it is in focus. These statements apply when the eye is accommodated for parallel rays, and they seem to be borne out by practical tests.

The explanation of the erect image obtained when the spot is held beyond the conjugate focal point of the eye, and of its

disappearance when the spot is held at this point, simply involves the crossing of the rays as in the case of the pin-hole experiment. These changes are most easily obtained with a convex lens, but it should have a diameter so large that it does not shut out any rays from the eye.

When the eye is accommodated for near points, it is no longer true that *FT* (Fig. 7) has practically the same illumination as before the interposition of the pin, but the general principle that the illumination of *FT* is less altered than that of *IN* holds good. When the eye is so strongly accommodated that the pin itself is brought into focus, *FT* becomes totally devoid of light and coincides with *IN*, which is now the sharply defined image of the pin, while the rest of the blurred image of the spot *AB* is unaffected in any way. When the eye is accommodated for a point between the pin and the eye the white shadow again appears, since *IN* now becomes larger than *FT* and the illumination of the latter is diminished less than that of its immediate surroundings.

If in place of a spot a black line is used, the pin will produce a white line running down the middle of its image; the shadow in this case is more marked and more easily obtained than when a small black spot is used. Two lines drawn at a slight angle to each other will give a bend in the white shadow at their intersection, but the shadow leaves one or both of the lines at a certain distance from this point. When a narrow red line is used instead of a black one, and a lens is employed to aid the eye, the white shadow takes on a greenish or bluish color. By the use of a lens a narrow line may be so blurred as not to be seen but a pin in front of the eye decreases so much the amount of light reaching the lateral portions of the image on the retina as to produce the appearance of a well-defined white shadow. The same thing is also true for a point.

Scheiner's experiment, in which two pin-holes placed close together are held before the eye and a double image of a pin is produced, may be explained in the same way as the white shadow just considered, since the two images obtained of the pin may be regarded as one image with a white shadow down its middle produced by the portion of the card-board between the two pin-holes.

When simply blurring a line by relaxing the accommodation, I have noticed that if the line is not too large I can always obtain a white line running down its middle and within this line a faint dark line. In the case of a small black spot, I obtain a white spot in the center of its blurred image. I think this must be due in most part to the denser and probably less transparent nucleus of the crystalline lens shutting off more rays than the rest of the refractive apparatus of the eye. This appearance is not produced by a photographic camera and hence must be mainly due to some such peculiarity of the eye as that suggested. It is possible, however, that the positive aberration of the eye may play some part in its production.

DIFFUSION OF THE MOTOR IMPULSE.

BY CLARK WISSLER AND WM. W. RICHARDSON.

The acquisition of voluntary control has always excited interest because of the peculiar problem presented and because of the feeling that its solution would put us far on the road to an understanding of the real nature of volition. It is a matter of common belief that we learn new movements by eliminating the unessentials from a veritable broadside of motor discharges, but science offers no satisfactory explanation as to how these unessentials are finally eliminated. It is considered logically impossible for voluntary to precede involuntary movement, since there must be a copy, or an idea, of the movement to be made before we can will to make it. Yet Baldwin insists that such a copy may be primarily visual, auditory, etc., instead of kinæsthetic, thus making imitation an important factor in the genesis of voluntary movement; but this copy is very imperfect and the corresponding kinæsthetic idea extremely vague, naturally resulting in a broadside of motor discharges, containing possibly the movements or some elements of the movements desired. This is the theory of excess.

It is quite certain that motor nerve elements evolve from the central system, and thus the order of development in a limb is toward the digits, or from fundamental to accessory. A great many writers have called attention to the phenomena of excessive or diffused movements when any part of the muscular system is working under great stress or extreme fatigue, or when executing unfamiliar movements; and it has been assumed that this diffusion is closely related to the so-called 'excess discharge' from which the more specialized movements are gradually evolved.

This paper is a report of some experiments with these diffused movements in normal unfatigued activity of the arm muscles.

The muscular anatomy of the arm is very complex, and though the positions of all the muscles are well known the action of many of them is yet a matter of dispute. Mechanically considered, the arm is a collection of levers capable of working in many combinations. This is the secret of its wonderful adaptability. One of the most complete series of levers is that presented when the arm, hanging freely at the side with the palm toward the thigh, moves in a plane parallel to the median plane of the body. In this position movements may occur at the shoulder, elbow (flexion), wrist (abduction) and at the knuckles (abduction of the first and second fingers). We have here a compound lever in which resistance could be applied at the end of the finger by movements of any one, any two, any three, or all of the parts in unison. All the levers in this series are represented by muscles at different levels, lying just beneath the skin, thus affording special advantages for observation. Beginning at the shoulder, the muscles chiefly engaged in the movements are the deltoid, biceps and brachialis anticus, extensor carpi radialis longior and extensor carpi radialis brevior, and abductor indicis.

The method of experimentation is quite simple and makes use of familiar apparatus. Tambours were placed over the muscles, so as to record their myograms upon a kymograph. To get myograms of the abductor indicis it was necessary to put the hand in a light clamping apparatus to serve as a firm support for the tambour. Some preliminary experiments revealed the fact that when the abductor indicis was contracted so as to move the unobstructed finger quickly through an arc of about 30° , secondary contractions of the arm muscles in the series occurred, but not at the same instant.

The limitations of apparatus made it necessary to study the muscles in pairs. Similar tambours were placed upon the biceps and forearm as previously stated. The time line was furnished by a 100-v. fork. The left arms of *R* and *W* were used in these experiments and records were taken at irregular intervals during a period of two months. The object was to determine the order of the secondary contractions of the two muscles of the arm as a result of the primary contractions of the abductor indicis. The abductor was not weighted, but simply

abducted the finger. The subjects made the contractions in response to a signal from the operator. Fig. 1 presents a typical

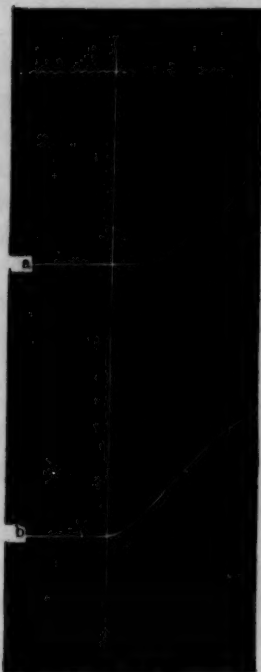


FIG. 1.

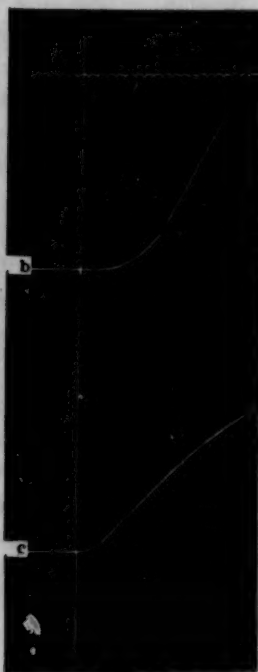


FIG. 2.

record. Here it is plain that the extensor muscles of the forearm contract sooner than the biceps. The average time difference for all the records was .016 sec. M.V. .004. In the cut, *b* is the tracing from the extensor carpi radialis longior, and *a* that of the biceps.

The tambours used in the above experiments were shifted to the forearm muscle and the abductor indicis of the same arm. Fig. 2 is a typical record. Result—the abductor indicis (*c*) is first in order of time. The average difference .017 sec. M.V. .005; the approximate difference between the abductor indicis and the biceps being .033 sec.

Incidentally, it may be observed that, if the tambour is placed upon the index finger near the end of the second phalanx, instead of upon the muscle itself, the extensor carpi radialis

longior contracts sooner than the movement of the finger. Average difference .02 sec. M.V. .005. Thus the difference in time between the beginning of the contraction of the abductor indicis and the movement of the index finger is approximately .037 sec. This represents the time required for the muscle to shorten sufficiently to pull on the finger. This result was further verified by placing one tambour on the finger and the other on the abductor.

In all of the above experiments special precautions were taken to secure accurate results. The point of contact on the arm was marked with ink, so that repetitions of the test could be made without variation. In order to eliminate possible error in the apparatus the tambours were tested as to readiness of response to pressure and were changed in order at various intervals during the experiments. In all, about 250 time measurements were taken.

From the foregoing it appears that when a motor discharge is directed to the extreme accessory muscle of the arm the law of diffusion is, primarily, to the muscle directly innervated, and, secondarily, to the adjacent related muscles in the order of their distance anatomically from the muscle innervated.

From all this it follows that exercise of the abductor indicis would train the other muscles of the arm, thus strengthening the whole. And then, if exercising the biceps will in turn train the muscles of the forearm and hand, we have evidence of a downward diffusion as well as an upward one. The following experiments were undertaken to test this inference.

The clamping attachment for the hand previously mentioned served for practicing the abductor indicis. In case of the biceps, the arm rested in a support with the forearm at an angle of 45° with the arm, permitting free flexion at the elbow; a small board was strapped to the inner side of the forearm, extending over the palm to the ends of the fingers. Attached to this board just below the wrist was a cord, pulling upon a dynamometer and also operating the recording point upon the kymograph. In this apparatus the forearm and hand became a rigid lever, pivoted upon the elbow and dependent for action upon the muscles of the arm proper.

When the muscles of the forearm were tested the arm was put in the same position as for the biceps, the forearm secured to an immovable board, and a shorter one placed upon the palm, extending from the wrist to the ends of the fingers, secured in such a manner that the only possible movement was flexion of the hand as a whole. A cord connected with the dynamometer as in the case of the biceps.

With this apparatus it was possible to get accurate records of the flexor muscles of the forearm, the biceps and the abductor indicis.

The first experiment was the training of the left biceps. The subjects were *R* and *W*, both young men in good physical condition, throughout the whole series. All records were taken in the laboratory from 8 to 9 a. m. Preliminary tests were made of the right and left abductors, the left forearm and the left biceps. On February 14th exercise of the left biceps was begun and continued fifteen days. The pulls were made in the ordinary counting rhythm. During the first five days 100 pulls were made daily without rest intervals, for the second five days 125 pulls and for the last five days 150 pulls. At the close of the practice period final records were taken of all the muscles previously tested.

RESULTS OF PRACTICE.

	Initial Resist. of the dynamometer.		Average Pull in mm.				Gain.	
	W.	R.	W.		R.		W.	R.
	Kilos.	Kilos.	Prelim.	Final.	Prelim.	Final.	g	g
1. Train. left biceps.								
" "	8.2	8.2	41.5	52.3	36.9	53.0	26	43
" forearm.	5.2	5.2	31.0	27.3	38.0	38.3	—	0.8
" abductor.	1.0	0.4	6.0	11.8	11.8	16.5	100	40
right "	1.0	0.4	5.0	6.3	14.0	17.0	26	21
2. Train. r. abd. indicis.								
r. abductor.	1.0	1.0	6.7	17.0	10.7	23.2	153	117
r. forearm.	8.2	8.2	14.0	30.8	21.8	32.7	120	50
r. biceps.	8.2	8.2	44.0	47.4	41.0	54.0	8	32
left biceps.	8.2	8.2	52.3	53.3	53.0	55.4	2	4
left abductor.	1.0	0.4	11.8	18.0	16.5	24.0	52	45

In order to increase the certainty of the results the other arm was practiced in reversed order. At the close of the above

series preliminary tests of the right arm were taken, and practice of the right abductor indicis begun March 8th and continued for fourteen days—100 pulls as before. Final records were taken for the muscles of both arms. The results for both experiments are given in the table.

As the table indicates, the training of the left biceps resulted in a gain of 26% for *W* and 43% for *R*. A defect in the adjustment of the recording apparatus, not discovered soon enough for correction, gave a very doubtful record for the forearm final, so the result in this case must not be set down as a positive exception. The unexercised left abductor shows a decided gain of 100% (*W*) and 40% (*R*). The right abductor made a gain of 26% (*W*) and 21% (*R*). We have here a measure of the reactionary effect of one arm upon the other, as well as that of a fundamental muscle upon an accessory.

The results for the right arm are quite significant. The exercised abductor indicis gained 153% (*W*) and 117% (*R*). The reactionary gain of the forearm muscles (120% for *W* and 50% for *R*), while large, was less than the gain of the abductor itself. This gain cannot be attributed to the direct exercise of the extensor carpi radialis longior, because with this apparatus movements of flexion only were made. The biceps, the most remote anatomically, gained correspondingly less—8% for *W*. and 32% for *R*. These results are in harmony with all our previous observations.

The records of the left, or resting arm, give the abductor a gain of 52% (*W*) and 45% (*R*) and the biceps 2% (*W*) and 4% (*R*). From this it appears that the accessory muscles of one side gain approximately as much from the exercise of the corresponding muscles of the opposite side as from the exercise of the fundamental muscles of the same side. That is to say, the diffused motor discharge from one side to the other is as great as that from the shoulder center to the finger center. The idea that training one muscle trains others is not a new one, as the various published observations of transference of practice from one side of the body to the other indicate.

It may be objected that the gains of the left arm, attributed to the exercise of the right, may be simply the result of resting

after a period of training; but the left biceps and the right abductor were the only muscles trained. The left abductor had no direct training, and shows a total gain throughout the entire series of about 300%.

In demonstrating diffusion by time relations the muscles were taken in series, but in these experiments this relation was disregarded and the muscles studied in groups, corresponding to the sections of the arm, so that the results are indicative of a more general diffusion than the preceding. *

From the above it seems certain that the exercise of any muscle reacts upon all other related muscles, which is to say that diffusion takes place in both inward and outward directions. Though the above quantitative comparison of gains indicates certain relations, yet our ignorance as to what are equivalent gains for different muscles makes it impossible to put forward the following in any other form than mere hypotheses:

a. That the exercise of an accessory muscle has a greater reactionary effect upon the adjacent fundamental muscles than upon the more remote.

b. That an accessory muscle of one arm gains as much from the training of the corresponding muscle of the opposite side as from the training of the fundamental muscle of the same side. In terms of motor discharge this indicates that these centers occupy the same diffusion level.

c. The reactionary, or secondary, gain of the fundamental arm muscles from the exercise of accessory arm muscles is less than when the conditions are reversed—*i. e.*, the fundamental muscles practiced and the accessory reacted upon. This is in harmony with the accepted order of motor development.

It is now time to consider the bearing of these results upon a few important questions. In the first place, it is desirable to know where this diffusion takes place—whether in the cord, the lower centers of the brain, or in the cortex. The fact that the order of diffusion is backward in the arm indicates that it is not due to the branching of the nerve, since in that event it would reach the nearest muscles first. If it is claimed that the impulse originates in all of these centers at the same instant, it can still be answered that the muscles slowest in response are anatom-

ically nearest the cortex. The biceps is innervated through the fifth and sixth cervical nerves, the extensor carpi radialis longior and brevior through the sixth and seventh and the abductor indicis through the eighth. (Quain's Anatomy.) So the muscles slowest in response are both in length of nerve tract and in relation to their innervating centers nearest the cortex. All this tends to show that the discharge starts down the finger tract first when its muscle is to be innervated, or in more general terms the discharge reaches the object of volition first. But it may be answered that we have here simply the differences in the latent times of the muscles, and that until it is shown that their latent times are equal no certain conclusion can be reached. This objection the present data cannot meet. A study of the reaction-times of these muscles may finally solve this problem. So we cannot yet say where this diffusion occurs, since the anatomical arrangement and our present knowledge of cerebral localization make it possible for it to occur either in the cord or in the cortex. Further research in the time relations of the expressive movements of facial muscles, accompanying states of physical effort, may throw some light upon this point. If it does take place in the cord, efferent diffusion is both upward and downward, according to the foregoing data, and not downward only as some physiologists have assumed.

The natural method of determining the order of downward diffusion would be with tambours, as in our first experiments; but this is rather difficult and will require new apparatus, so that no results can be given at this writing.

Our experiments in training were undertaken to show that diffusion took certain directions, but it is well to view the results in relation to previous research in 'cross-education' and transference of practice effects from one side of the body to the other. Two very recent publications by Dr. R. S. Woodworth¹ and Walter M. Davis² have so completely summed up the past and so well advanced our present knowledge that we shall discuss our results in relation to them alone.

¹ Accuracy of Voluntary Movement, *PSYCHOLOGICAL REVIEW*, Supplement, Vol. 3.

² Cross-Education, Studies from Yale Psychological Laboratory, Vol. 6.

Dr. Woodworth gave some attention to the transference of accuracy of movement from one side to the other. He says: "These results show (1) that the transference of the effects of practice from one side of the body to the other—a transference which has been established in other investigations as taking place from the right side to the left—also takes place from the left side to the right, and (2) that it is not the mere practice that has this transferred effect, but only successful practice. * * * Where the left hand does gain the right shares the benefits in almost equal measure."

Mr. Davis gave his entire attention to the transference of practice effects. He finds that resistance to fatigue and the size of muscles increase on the unpracticed side. In brief, he finds the following: "(a) The effects of exercise may be transferred to a greater or less degree from the parts practiced to other parts of the body. This transference is greatest to symmetrical and closely related parts. (b) There is a close connection between different parts of the muscular system through nervous means. This connection is closer between parts related in function or in position."

In one part of his work Mr. Davis made experiments similar to our own. He trained the biceps with dumb-bell exercise and recorded the gain in grip, assigning as a cause of the latter the observed clenching of the hand due to the onset of fatigue. Our experiments show that there is an actual contraction of the related muscles of the same side corresponding to the observed practice effects; and, since the practice effect appears upon the other side also, it is natural to infer that those muscles contract. As previously shown, it is possible for a muscle to make an appreciable contraction before movement of the limb occurs. For this reason it must not be claimed that muscles do not receive cross-exercise until more refined methods of observation are used than heretofore in such experiments.

There is a disposition among writers to explain 'cross-education' as a change in the motor centers of the opposite side, resulting from the close functional relation of the two halves of the brain, but this is upon the assumption that the muscles of the opposite side do not contract. Even with such an assumption

it is difficult to see how a change, resulting in a contraction, could be transferred to another cell without resulting in a contraction of the muscles with which this second cell holds functional relations. So far, all results are in harmony with the explanation of the case by diffused motor currents. In our experiments the gains of the muscles receiving the diffused currents were less than the gains of those directly innervated, and in the researches just mentioned the gains of the unpracticed side were less than those of the practiced side. Naturally as the current spreads it gets weaker and weaker. The question can be definitely settled by placing delicate tambours upon the muscles of the opposite side. It would also be interesting to try the practice effect in simply thinking the movement.

On the whole, it seems that any attempt to explain the transference of practice from one part of the body to another must consider the muscle, nerve and center as a unit. Of course, we are speaking of gains in strength and endurance only; gains in precision may hold different relations to the mechanism.

In conclusion this study leads to the belief that there is a diffusion of the motor current in the arm, and that it follows out an order corresponding to anatomical and functional relations such as would occur in an irradiation of the current in the cells of the cortex or in the spinal cord. Also, it seems to follow the lines of anatomical and functional development, the accessory motor tracts being so intimately connected with the fundamentals that there is a constant leakage of the current into these old channels. By further research we hope to reach more specific conclusions.

We wish to express our indebtedness to Dr. J. A. Bergström, Indiana University, for some valuable suggestions in the formative stage of the work and for experience gained from assisting him in some yet unpublished research with the ergograph. The experiments here reported were made in the psychological laboratory at the Ohio State University.

THE COLOR CHANGES OF THE WHITE LIGHT AFTER-IMAGE, CENTRAL AND PERIPHERAL.

BY DR. MARGARET FLOY WASHBURN.

Welch College.

Two problems were investigated in the observations described in this paper. The first concerns the dependence of the series of colors seen in the white light after-image upon the duration and intensity of the stimulus. It is well known that the flight of colors in an image produced by prolonged stimulation with ordinary daylight differs considerably from that occasioned by shorter or by instantaneous stimulation, but the relation of these different color series to each other has not yet been made out. For about a year and a half I have been studying this relation by a method of experiment which I believe to be new: the method, namely, of overlapping two images in such a way that the portion of the retina which corresponds to the overlapped part of the image shall have been stimulated twice as long as the portions corresponding to the rest of the image. The effect of increased duration of the stimulus may be studied in this manner far more conveniently than when the comparison has to be made between successive images.

The second problem is one that, in the form in which it is here investigated, has received little attention from writers on the subject of after-images. It concerns the series of colors to be observed in an image which falls towards the side parts of the retina. The observations here recorded represent the merest beginnings of such a research: they have resulted in the establishment of only a few scattered facts, but they are perhaps worth publishing if they suggest a field of study to workers who have better opportunities for investigation and fuller theoretical knowledge of the subject than the present writer possesses.

I. THE DEPENDENCE OF THE COLOR SERIES ON THE DURATION AND INTENSITY OF THE STIMULUS.

If one looks steadily at a bright white sky, *i. e.*, a sky covered by a thin uniform cloud, or at a sun-illumined snowfield, for about fifteen seconds, and if the eyes are then closed and covered with the hands, the phenomena observed will be as follows: Just as the eyes close there will be a short after-image of the window or the luminous object; this will usually disappear, and after a few seconds a bluish white image will be seen, deepening to a rather bright light blue. A vivid green color succeeds, lasting a long time, and at length giving place to red. The red image is succeeded by a dark blue image, and this again by a very dark green image, whose color is sometimes scarcely distinguishable. If the image is that of a cross-barred window, the change from the positive to the negative image, as indicated by the brightening of the lines representing the bars, begins at the end of the first green stage, and is complete about the middle of the succeeding red stage.

This series, then, blue, green, red, blue, green, is the color series which occurs for maximum duration of the stimulation, and for the maximum intensity obtainable by ordinary daylight. I shall, therefore, call it the maximum color series. Increasing the duration of the stimulus produces no change in the above sequence of colors. It is identical with that observed by Helmholtz and Fechner under the same conditions. The blue and green of the final stages are darker than the earlier blue and green, which are also less saturated, suggesting that the latter are accompanied by a brightness process which diminishes in intensity as the image runs its course: otherwise, the color series looks like a circular one, and indicates that if the image lasted long enough, we should have a second red stage, completing a second cycle of blue, green, red.

Now if it happens that the field of stimulating light is of different intensity or brightness at different parts; if, for instance, we have an irregularly clouded sky, we find, as Helmholtz observed in the after-image of the sun's disk, that the more intensely stimulated parts of the image take longer to go through

this succession of colors than the less intensely stimulated parts. When the latter have passed into the first green stage the parts corresponding to brighter spots in the stimulating field persist as spots of blue; these spots remain green after the rest of the image has turned red, and so on, persisting in each color stage after the surrounding parts of the image have entered the succeeding stage. Thus we see that after the maximal series has been reached increase in the intensity of the stimulating light increases the duration of the several color stages in the series.

Now by the method of overlapping images it can be shown that increasing the duration of the action of the stimulating light has the same effect as increasing its intensity. The overlapping of the images can be very easily done by first fixating one point on the bright surface for, say, fifteen seconds, closing the eyes for an instant, and then fixating a different point on the stimulating surface for fifteen seconds. Two images will then be found, which will partially coincide or overlap; and the portion of the retina corresponding to the overlapped part of the image will have been subjected to a stimulus of twice the duration of that which has affected the parts covered by the non-coincident portions of the two images. Now when the brightness of the light is such as to give the maximal color series after a fifteen-second exposure, the overlapped parts of the two images, which have been subjected to a stimulus of thirty seconds' duration, behave just like the images produced by intenser stimulation. That is, when the other parts of the two images have passed into the first green stage, the overlapped part remains blue; when they have become red it remains green, and so on; in other words, when the maximal series has been reached, further increase in the duration of the stimulus increases the duration of the color stages in the series.

Now when the intensity of the illuminating light is less than we have supposed it to be in the experiments just described; when, for instance, the sky is rather heavily clouded; or when the duration of the stimulus is reduced to seven or eight seconds, a different series of colors is observed. The image is at first blue; then it passes through a momentary nearly colorless stage into red; this is followed by the darker blue stage and this again

by a very faintly greenish black. The characteristic difference between this series and the maximal series is that the first green stage is dropped out: the image passing either directly or through a very short period of colorlessness from blue into red. This fact also was noted by Helmholtz. If certain parts of the stimulating field are brighter than the rest the corresponding parts of the image pass through the green stage, while the surrounding portions omit it. And if two images be overlapped as described above, the overlapped parts show the green, while the other parts do not. The conclusion to which these phenomena lead is that the first qualitative effect of diminishing the intensity or duration of the stimulus is the dropping out of the first green stage in the color series. The total duration of the image is also, of course, shorter.

In the next place, if the intensity or duration of the stimulus be still further diminished; if, for instance, the sky be looked at for not more than five seconds, the sequence of colors undergoes another change. The image is blue or violet at first, passes quickly from violet to red, and from red into a greenish black. Between the red and this final stage there is often noticeable a stage of washed out or indefinite color, which Helmholtz calls 'dull orange,' but which is not unlike the colorless phase mentioned above as taking the place of the first green image. The violet seems to be due to the rapid transition from blue into red. Now when two images of this sort are overlapped in the usual manner, the overlapped parts show the second blue stage, which does not appear on the remaining portions. It is evident that the next qualitative effect of diminishing the intensity or duration of the stimulus is the dropping out of the second blue stage from the color series.

The progressive effect of diminishing intensity or duration might then be stated as follows: Shortening the stages of the maximal series (blue, green, red, blue, green), dropping out the first green stage; shortening the stages of this second series (blue, red, blue, green), dropping out the second blue stage.

My observations show that the minimum of intensity and duration which will give a colored image at all, results in a violet-red image followed by a greenish-black image, the violet-

red seeming to be the effect of the rapid transition from blue to red. It must be borne in mind that these experiments show that intensity and duration of stimulus have precisely the same significance for the color changes of the image: that is, a thirty-second exposure to moderate light produces the same effect as a fifteen-second exposure to stronger light. Thus when we find in Helmholtz or elsewhere, a statement of the color series resulting from 'instantaneous illumination' we must know the intensity of the illumination before we can understand the significance of the series. A stimulating field that, like the sun's disk, gives different brightness at different parts may yield results of great complexity, such as those described in a footnote by Professor Külpe (*Outlines*, p. 131); but my own observations show these results to be in accord with the general laws above stated. Practice will enable one to distinguish, from the colors of the image at a given instant, which parts of it were produced by intenser stimulation and which by duller light.

To present a theory accounting for the observations described above is not possible, at least for the present writer, while the physiological significance of the flight of colors is so imperfectly comprehended. One or two theoretical points do, however, seem to be fairly deducible from these facts. First, the fact that the colors in the earlier stages of the image are so much 'lighter' and less saturated than those in the later stages suggests that the color processes of the image are accompanied at first by a colorless process, which gradually diminishes in intensity as the image runs its course. Second, if we adopt the ordinary theory that the series of colors is due to the fact that the various color processes set up by the action of white light on the retina differ in the rapidity with which they lose intensity on the one hand and the length of time during which they persist on the other, then we must suppose that the degree to which color processes differ in these respects is a function of the intensity of the stimulating light. For instance, if the intensity is diminished, the difference between the green process and the blue or red process is abolished and the image passes from blue into red without exhibiting an intermediate stage. Such a supposition meets with many difficulties. But, thirdly, it is hard to reconcile the current

theory, as stated above, with the observation that the series of colors in the image is a series that tends to repeat itself. Blue, green, red, blue, green, is the full color sequence, and the later blue and green stages differ from the earlier, to all appearance, only in intensity and saturation. Now, why, if the red color is due to the fact that the other color processes have been exhausted, should it be followed by a revival of the exhausted processes?

II. THE COLOR CHANGES OF THE PERIPHERAL WHITE LIGHT IMAGE.

Aubert, I believe, made the most systematic observations on record of after-images produced on the side parts of the retina (*Physiologie d. Netzhaut*, pp. 375 ff.). The white light stimuli which he used were electric sparks, and he describes the image as invariably colorless even within the zones of possible color stimulation. It occasionally looked larger than an image seen in direct vision. This fact he thinks due to imperfect focussing of the image. Even when colored glass was interposed before the stimulating light the image was colorless. Aubert also experimented with the negative images of small squares of colored paper seen indirectly. He found that the negative images had the color which they would have had in direct vision, and that they were of shorter duration than central or foveal images. As to their relative size compared with direct vision images he says nothing.

The experiments whose results are discussed below do not involve accurate quantitative determinations, exact measurements not being possible with the apparatus at hand; and the few facts gathered from them may be easily verified by any observer. The light used was white daylight, of an intensity and duration sufficient to give the maximal color series in a central image. The width of the image, that of a square opening in a black window screen, corresponded to about $1\frac{1}{2}^{\circ}$ on the retina. It was observed at distances from the fovea of between 5° and 15° .

1. The first noteworthy observation is that the peripheral image is smaller than the central image. This is readily seen

if light be admitted into a darkened room through two openings of the same size, the observer fixating one of these openings. The after-image of the indirectly seen opening is noticeably smaller and narrower than the image on the center of the retina. Why this should be so becomes evident from a study of the diagram showing the projection of the Helmholtz Circles of Direction.

2. The positive white-light image which is located outside of the macula lutea is practically colorless in the darkened field of the closed and covered eye. Sometimes a faint green color is distinguishable, but, in general, I have found Aubert's statement as to the colorlessness of the image confirmed.

3. The negative image of a white light, indirectly seen, is as Helmholtz says, very difficult to observe *in the darkened field*. Usually a long interval occurs between the positive and negative images. But when it comes it is always *colored*, a violet red, often scarcely distinguishable from the dark field, but unmistakable in color when seen. This, again, is consistent with Aubert's observation that the negative images of colored papers are distinctly colored on the side parts of the retina.

The foregoing observations were all made in the darkened field of closed and covered eyes. If the peripheral image be examined when light is let in through the closed lids, two curious and interesting phenomena occur. These may be studied best when simultaneous peripheral and central images are produced by the method suggested above.

4. If the two images be watched in the darkened field, the central image will have the usual color, and the side image will be colorless. But if light be admitted by removing the hands from the closed eyelids, the side image will flash out into a peculiar brilliant green, like a bit of stained glass, and giving the same transparently luminous effect. The foveal image does not seem to give this phenomenon at all with ordinary intensities, but a similar appearance may be observed in the foveal image of the sun's disk. The fact that the green color is seen by transmitted light suggests that it is a fatigue phenomenon, and one that appears in peripheral images more readily, *i. e.*, at a lower intensity of the stimulating light, than in central images.

5. The second observation to be made when light is admitted into the field of vision concerns the duration of the effect of fatigue. When peripheral and central images are produced simultaneously in the manner described above and studied in the darkened field, the side image disappears before the central one; as Aubert says, the peripheral image is of shorter duration. The difficulty of keeping the attention fixed on a faint object towards the side of the field complicates the observation here. However, after both images are practically lost in the darkened field, if light be admitted through the lids or by opening the eyes, the peripheral negative image is visible as a black spot long after all trace of the central image has gone. Nothing could be more strikingly evident than this phenomenon. I have found the difference in duration to be as great as five minutes, and have no doubt that it was often more. The colored image seen in the darkened field, which I have above called a negative image, disappears sooner if it falls towards the side regions of the retina. But the black image seen when light enters the field lasts longer when it is situated towards the periphery. And this image is necessarily and unequivocally a fatigue phenomenon. The conclusion suggested is that the effects of fatigue are more lasting towards the side portions of the retina than near the center.

One more comment remains to be made on the results of these experiments. As has been already said, the unsaturated character of the colors in the early stages of the foveal image suggests that an achromatic or brightness process accompanies the color processes at first, becoming less and less intense during the course of the image. May not this supposition explain the fact that the early stages of the side image are colorless or very nearly so (Aubert says 'faint yellowish'; I should call the color greenish, where any color can be seen), while the later stage is distinctly colored violet red? The ratio of the colorless process to the color process would naturally be greater towards the side of the retina, and during the first stages of the image the color process might be wholly swamped. Later, when the achromatic process had diminished in intensity, the color stage would become visible.

THE PSEUDOSCOPE AND SOME OF ITS RECENT IMPROVEMENTS.

BY PROFESSOR JOSEPH JASTROW.

University of Wisconsin.

The pseudoscope forms the complement of the stereoscope; the latter demonstrates the significance of the several factors in the perception of the third dimension of space; the former completes the demonstration by showing that the interpretation of positions in the third dimension may be inverted by interchanging the points of view of the two retinae. In both cases the success of the demonstration depends upon the suitability of the instrument, as regards convenience, precision, accuracy of adjustment to different eyes, and the naturalness of the viewing process. The selection of the object viewed is also an influential factor. There are thus many kinds and degrees of stereoscopic as of pseudoscopic effects. In another connection (*Science*, May 6, 1898), I have reviewed various forms of the stereoscope and of stereoscopic accessories which are of service to the psychologist in the demonstration and explanation of this most important group of psychological inferences. I desire now to notice in a similar way some inventions and devices in the field of inverted stereoscopy, or pseudoscopy, if the term be allowed.

By far the most important of such inventions is the new form of pseudoscope, designed by Professor R. W. Wood, of the University of Wisconsin, and described in *Science*, November 3, 1899. The accompanying cuts, Figs. 1 and 2, illustrate the form and principle of the apparatus. Its essential parts are a pair of stereoscopic prismatic lenses, mounted just as in the stereoscope, and a pair of ordinary double convex lenses held at a suitable distance in front of these.

The inverted and reversed images of the object viewed, formed by the outer pair of lenses, are in turn combined (and

enlarged) by the stereoscopic lenses. Professor Wood thus illustrates this principle: "Let A and B be two points in space, B being in front of A . An eye at X will see B to the right of A , and an eye at X' will see B to the left of A , and the fusion

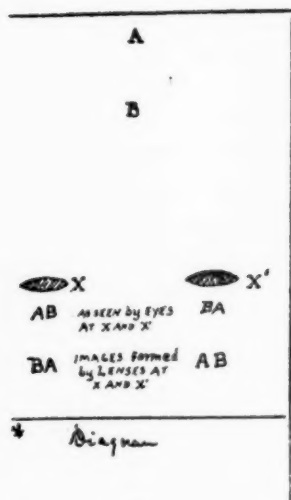
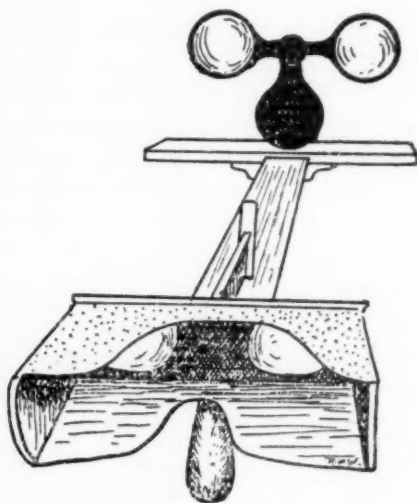


FIG. 1.



A NEW PSEUDOSCOPE.
Figure -

FIG. 2.

of these two images produces stereoscopic vision, B appearing nearer than A . Suppose now that X and X' represent the two lenses. The images which they will form in space will be reversed [and inverted, although for the sake of simplicity they are drawn erect], that is, the lens X will give an image in which B will be to the left of A , or just the opposite of the appearance presented when the eye is at X . It is apparent that the images BA and AB formed by the lenses are identical with what would be seen by eyes at X and X' , provided A were in front of B , consequently the fusion of these two images makes A appear nearer than B ."

To perfect the instrument, especially as regards convenience of use, the outer pair of convex lenses may be set in tubes at

the outer end of which are the stereoscopic lenses; the one pair of tubes slides within the other, thus permitting the distance between the two lenses on each side to be varied. The apparatus thus takes the form of a small double telescope, or an elongated opera-glass, and is now being constructed in this form.¹ It should not be lost sight of that this method of securing the pseudoscopic effect is entirely different from any other.

In practice, the Wood pseudoscope gives more striking results than any which I have used, and promises to become the standard form of the instrument. It accomplishes the inversion of suitable objects with a vivid likeness to reality that is startling. The objects may be small or moderately large, and in any ordinary position and at a moderately variable distance; it may be used much like an opera-glass, except that it is limited to objects at fairly close range. As to the objects, simple geometrical shapes with rounded contours are to be preferred; bowls, glasses, turned forms with depressions and elevations, mathematical models, and so on, may be recommended. But complex forms are, by no means, excluded; even such as a typewriter or the view of the miscellaneous objects in a room or a street view. But in these a larger number of opposing factors enter, and the desired results cannot always be secured. The inversion suggested by the interchange of retinal relations is opposed by the conflicting evidence of light and shade, of the interposition of objects, of the unnaturalness of the entire result. Thus it is difficult to see pseudoscopically the human face: here apperception constitutes an influential factor, and when the suggested inference goes against the grain of experience, the suggestion fails or is not completely realized. But the observer may notice some effect of this rivalry of motives, to use Professor Stratton's apt phrase, the pseudoscopic view 'saps the life' out of the true perspective, even when it does not replace the latter. The variety of interesting pseudoscopic effects—the manifold transformations of cameo to intaglio, of foreground to background, of advancing contours to receding ones—has not been sufficiently appreciated, and mainly because of the

¹By the Chicago Laboratory Supply Co., who will probably be prepared to furnish the apparatus.

rather unsatisfactory kind or limited range of such effects which the ordinary forms of pseudoscope have yielded. To a large degree, these disadvantages have been overcome by this new form of the instrument.¹

In this REVIEW for November, 1898, Professor Stratton describes a mirror pseudoscope of novel design and valuable alike for its illustration of the pseudoscopic principle, and for its variability of application and consequent aid in the analysis of the component factors of the pseudoscopic effect. One of these variations allows of an exaggerated pseudoscopic and another of an exaggerated stereoscopic effect, both of which advantages

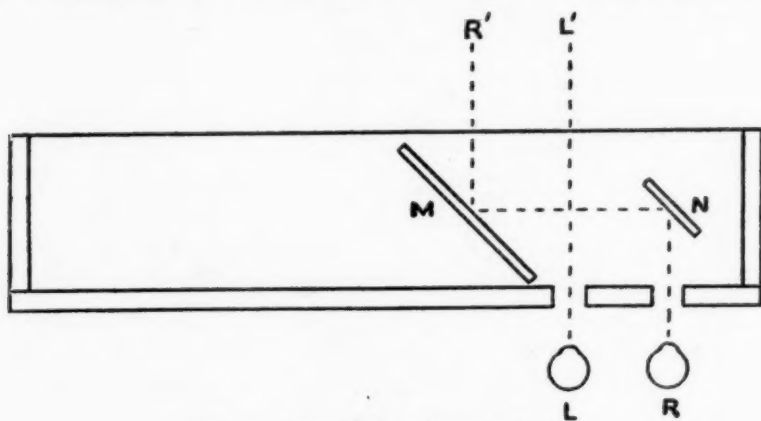


FIG. 3. Normal Pseudoscopic Vision.

are possessed only by the Stratton pseudoscope. Professor Stratton has given an able description of the nature and applications of this instrument, which may be referred to for further details. The accompanying cut (Fig. 3) illustrates the principle by which the right eye obtains a 'left-hand' view and the left eye a 'right-hand' view of the object to be seen pseudoscopically.

¹ Although the announcement is somewhat premature, I am tempted to add a word regarding an application of the Wood pseudoscope, which I am now contemplating. I am studying the capacity to judge of the relative convexity of a series of convex forms of variable convexity, viewed under a uniform shadowless light. It would be difficult to apply the test to a similar series of concave forms because the receding parts of the concave forms would be in relative shadow; but by viewing the convex forms with a pseudoscope the desired result is attained most readily. An account of these experiments will be published in due season.

I cannot infer with certainty from Professor Stratton's description, whether he regards that the mirror form of pseudoscope is itself novel or only his form of it. At all events, there is a mirror pseudoscope designed by Professor Ewald, and made by Major, of Strassburg, and the Laboratory of the University of Wisconsin possesses such an apparatus. The setting of the mirror and the path of the rays by which the 'left-hand' view of the object reaches the right eye and the 'right-hand' view of the object reaches the left eye can be readily understood by reference to the accompanying figure (Fig. 4). In practice the instrument is extremely limited in scope. The objects to be viewed must be small and practically at one distance only; and even then only a measurably satisfactory effect is obtained, by no means comparable in life-likeness to that of the Wood pseudoscope. Professor Stratton's arrangement of the mirrors is in every way more advantageous and makes the mirror pseudoscope a valuable form for many purposes.

The inversion by total reflection in prisms which was the original form of the Wheatstone pseudoscope, has thus been added to by (*a*) inversion by reflection in mirrors, and (*b*) by inversion by fusion of the inverted images of convex lenses. To complete the review of the pseudoscopic method I give the principle of the Wheatstone total reflection pseudoscope from Sanford's *Experimental Psychology*, pp. 280-281. (See Fig. 5.)

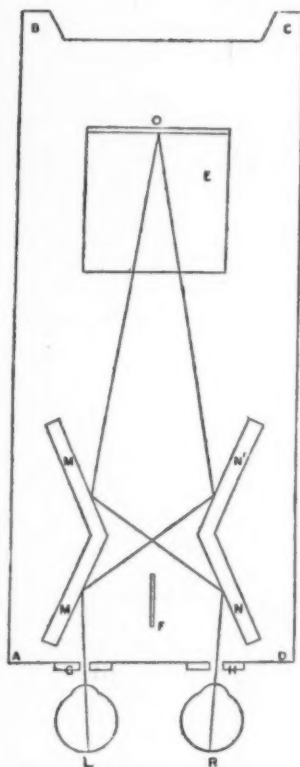


FIG. 4. *ABCD*, the board on which the parts are mounted. *E*, a mirror and screen to regulate the light. *F*, a screen to prevent the eye from seeing both mirrors. *G* and *H*, openings for *L* and *R*, the left and right eyes. *O* is the object viewed.

One other form of pseudoscopic effect remains to be noticed. namely, the familiar one of interchanging the pictures of the

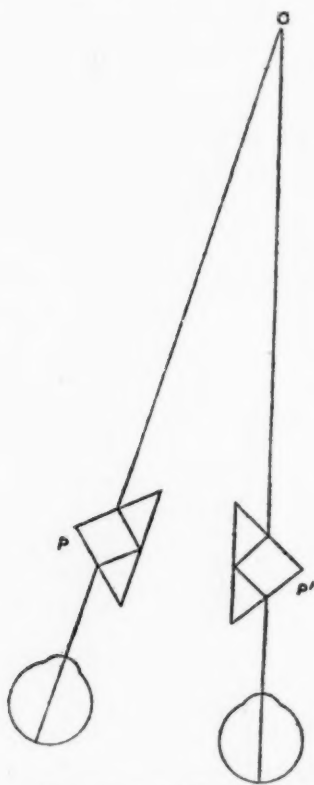


FIG. 5. *R* and *L*, the right and left eyes. *P* and *P'*, the prism. *O*, the object. "The hypotenuse of the prisms practically acts as a mirror, reversing the view of the object; the direction of the ray is unchanged. Each eye thus sees a reversed view of the object."

ordinary double stereoscopic photograph. The most suitable objects are geometrical diagrams, and of these by far the best is the series of mathematical curves of motion, photographically produced by Professor Schlichter and referred to in my article (*Science*, May 6, 1898). The most satisfactory method of securing the inverted effect is by use of the modification of the 'perspectoscope,' which I described in the same article. I have modified the instrument by pivoting the mirrors instead of retaining them in fixed positions. Setting the mirrors so that when viewing the ordinary stereoscopic card, I see the mathematical curve or photographic picture in true perspective (*i. e.*, the right eye sees the right-hand picture and the left eye the left-hand picture), I have only to alter slightly the angles of the mirrors so that the reflection of the left-hand picture is seen by the right eye and that of the right-hand picture is seen by the left eye, and the reversal of perspective is at once accomplished (Fig. 6). Not only can the same card be used for stereoscopic and pseudoscopic purposes

by means of this instrument, but one may pass to and fro from one to the other effect at will and with but a slight movement of the mirrors. I am now arranging such an apparatus so that the mirrors naturally stop at the two positions desired.

In instruction in psychology, the analysis of the factors of

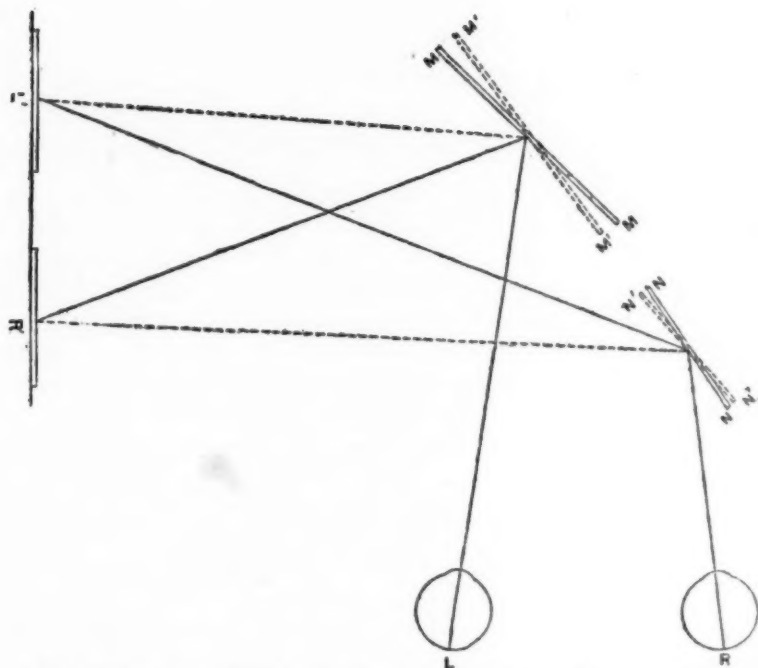


FIG. 6. R and L , the right and left eyes; R' and L' , the centers of the right and left halves of an ordinary stereoscopic card; MM and NN the mirrors pivoting on their centers. When the mirrors are in the positions of $M'M'$ and $N'N'$ R sees R' and L sees L' , as indicated by the dotted lines, and the ordinary stereoscopic effect results. When the mirrors are set in the positions MM and NN , R sees L' and L sees R' , as indicated by the dotted lines, and there is a pseudoscopic effect.

depth-perception is so helpful to a right understanding of the nature of a 'perception-complex,' and furnishes so useful an illustration of many psychological principles, that all devices that contribute to the clearer and more convenient demonstration of the several elements of the complex, should find a welcome place in the psychological laboratory.

DISCUSSION AND REPORTS.

PROFESSOR STUMPF ON EMOTION.

Stumpf's attractively written and well-constructed article, 'Ueber den Begriff der Gemüthsbewegung,' a notice of which appeared in the November number of this REVIEW, deserves, I think, some further discussion; for its tendency, as it seems to me, is clearly to bring together the two main lines of divergence in the controversy on the theory of emotion. Stumpf himself, to be sure, undertakes to defend the common doctrine as over against the 'sensualistic' theories of Ribot, James and Lange; but in the end he makes such large concessions to these theories that we are enabled at once to judge of the extent of their influence in modifying the traditional conceptions, and also to see more clearly what is yet needed in order to bring about something like substantial agreement. Stumpf defines an emotion (*Affect = Gemüthsbewegung*) as 'a passive state of feeling relating itself to a judged situation' (*Sachverhalt*). He considers the judgment—the term taken in the widest sense—essential, not merely as a condition, but as an integral part of the emotion itself. Assuming that the term 'passive' is intended not to exclude all motor tendency, but merely to mark off in a practical way the emotions as such from the impulses as such, I should myself, though holding in general to the 'sensualistic' hypothesis, heartily accept this definition. As to the further statement which makes the judgment a constituent part of the emotion, more will be said later. Stumpf criticises Ribot for identifying emotions with sensation-feelings; he criticises James and Lange for refusing to recognize an affect-process distinct from the sensory report of muscular and organic disturbance. In the extended criticism of the James-Lange hypothesis which occupies the larger part of the article, there is, of course, much repeated that has been said already, and at some points the criticism, as usual, would seem to rest on misunderstanding. In reply, for example, to the argument that if we abstract in our thought of a strong emotion from its organic 'accompaniment,' nothing of the emotion is left, we are told that the chief ingredient in our thought of an emotion is not organic phenomena at all, but visual imagery, *e. g.*, red or pale cheeks,—as if James in this

famous argument were referring to our representation of another's emotion rather than our own! But there is very effective criticism of the pathological cases usually appealed to, especially of Sollier's, and along with this a very proper refusal, I think, to recognize as an emotion in the strict sense any feeling which is objectless. There is also a noteworthy criticism of the so-called law of dynamogeny, regarded as affording a general support for James' theory. The criticism is specially directed against Féré, whose well-known results are strongly suspected and on the best showing regarded as inconclusive for the establishment of a general law. The 'motor' discharges, it is urged, need not pass to the periphery at all, but may remain latent as tensions in the brain till released by new stimuli, or if they do pass to the periphery they may be too weak to be felt. The insistence here, of course, is on the law of the threshold. Unfortunately, this criticism too, interesting as it is in itself, hardly affects James' theory; for although James does hold that every idea is motor in its consequence, he does not claim that every idea necessarily arouses an emotion. Nor do I find specially convincing Stumpf's restatement of the direct arguments against the theory, namely the failure of emotions to correspond in quality, intensity and time-sequence to the bodily changes said to constitute them. It is hard, for example, to believe that the 'tiefe Rührung' inspired by a deed of heroism or the 'Ergriffenheit' of a highly sensitive nature in presence of an artistic masterpiece bears, as Stumpf asserts, no correspondence whatever to increase of pulse, dilation of blood-vessels, etc. It seems to me that we have in this field still little more than dogmatic assertions on both sides. We need more experiment. But even experimental results will be of little avail unless we know how to interpret them. And here the difficulties are great, not to say insuperable. When, for example, we seek to determine the relation of the intensity of the emotion to its expression, what precisely do we mean by emotional strength? One popular measure of the strength of an emotion is its persistence. But here we tend to pass from the emotion as a conscious state to the emotional disposition, the organized capacity for specific feeling. Evidently it is not this, but only some actual passing phase of the emotion which we can attempt to measure. But how can we determine anything about the intensity of even that? Will our subjective appreciation of its intensity suffice? Then we might, indeed, expect to find very frequently a decided lack of correspondence between the intensity of the emotion so measured and the evidence of our kymographic tracings, for our subjective conviction of the intensity of an emotion is measured on com-

plex standards of value which no actual physical measurements can ever possibly reach. But would this prove the latter altogether fallacious? The question is a highly debatable one; yet surely there must be something in consciousness corresponding to these physical magnitudes, and how far that something is to be regarded as emotional will depend on the general results of psychological analysis and on general theory as to how an emotion is constituted. In principle, at any rate, the difficulty of measurement is no greater here than in the simplest cases of psycho-physical experiment: we obtain in them a measure, if not the sole measure, of psychical excitement, and control the differences of subjective appreciation by allowing something to the method of averages.

But the significance of Stumpf's article, as it seems to me, lies in its recognition of the value of the 'sensualistic' theories and in the concessions made to them. It recognizes their value, namely, in emphasizing the organic sensations in the description, as distinguished from the definition, of the emotions, in pointing to new classifications of them, and in making clearer the conditions of their genesis. And it concedes not merely that the affect-process may in some cases follow, instead of preceding or accompanying, the organic discharges, but that organic sensations are part-contents, and not bare accompaniments of the emotions, whose quality they profoundly affect. Are we not here appreciably nearer to a common understanding? The first thing to be recognized is that James' 'theory' has nothing whatever to do with the logical definition of an emotion, but with its psychological analysis. James' question, *What is an Emotion?* meant, as the whole course of the discussion shows, not, how would you abstractly define it, but what is the content discoverable in your consciousness when you experience it. In other words, the question related to the nature of the experience when examined from the psychological point of view as a content of consciousness. From this point of view definition is description. And surely the psychologist, at any rate, must recognize that the determination of the '*Begriff* der Gemüthsbewegungen,' however important in itself, is after all but a preliminary to the analysis of the experience regarded as falling under the conception. Now Stumpf admits that organic sensations are part-contents of an emotion and commends the 'sensualistic' theories for forcing us to assign to them a much larger part in constituting the experience than is usually done. He further recognizes, as we have seen, that judgment of the relation of the feeling to an object is also an integral part of the experience. He is far, therefore,

from agreeing with Dr. Irons, for example, in thinking that the emotional experience is psychologically unanalyzable. Assuming then that the question is one of psychological analysis, we seem to have here a distinct approach to an agreement. The organic sensations are admittedly part of the emotion; are they not perhaps the dominant and determining part? There are two further questions. The first is this, Is Stumpf right in making the judgment part of the emotion? It would be better, I think, to say, the feeling of the relation to the object. This feeling I hold is always present, qualifying the feeling arising from organic disturbance, so much so, indeed, that when the latter is isolated and regarded simply as a sum of sensations its distinctively emotional character disappears. This may reasonably be conceded. And this brings us to the second question, Is Stumpf's criticism sound, that the James-Lange theory refuses to recognize a distinct affect-process? If what has just been said is true, then we must admit that it is. The distinctive character of the emotional process as such lies precisely in this feeling of the relation of our state to an object. Consciousness is fundamentally feeling; all changes of consciousness are changes of feeling and in emotion proper the relation to the object is clearly felt. Hence, as has often been pointed out, when attention is specially directed to the bodily sensations present in the actual emotional experience the felt organic relation of the elements of our analysis is destroyed, the emotion tends to evaporate and the sensations stand out clear and distinct for themselves. The organic relation of elements in the emotion has its bodily condition, possibly in some associative central function, but it is no doubt there.

The James-theory, so-called, is really a theory and an analysis. Its analysis is imperfect, as Stumpf and others have shown; but it is at least interesting to see so distinguished a critic acknowledging in effect how nearly it comes to being right. The theory that the reaction consisting in bodily discharges, the feeling of which is essential to the constitution of the emotion, is instinctive, has never, I believe, been successfully assailed, though the only attempt to appreciate it, so far as I know, is in the penetrating articles of Dewey, which appeared in these pages several years ago.

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THE GENESIS OF GENERAL IDEAS FROM GROUP PERCEPTION.

By group perception we, of course, do not mean aggregate perception. Thus, if my first experience with oranges is in seeing a dozen on a table, I apprehend these as a group, and not as an aggregate. Mere aggregate vision—and so multiple image in re-presentation which is plainly not general idea—may occur with animals having multiple eyes; but in ordinary single vision the group is grasped together as such, and so in re-presentation the single image embraces the many like things, giving thus a general idea. The empiric germ of the general idea lies then in apprehension of group of several or many; and this group is a common circumstance in actuality, and a necessary one to be grasped by successful adaptation in the struggle of existence. That a mere aggregate of pictures may imperfectly fulfil this function with multiple eyed animals seems likely, but in higher forms single image vision achieves a picture of aggregates in practically simultaneous perception, for example, a number of yellowish roundish objects—oranges—here is real presentative concept of group of the several made in a single focus, and also by the panoramic sweep of the eye which implies use of after-image in the construction of the group. In this way if we first see negroes in a group, we note them as 'blacks,' and refer after-experiences to group 'blacks.' Here by direct inspection objects are seen together as a group, and if next day I see a negro the presentative concept of the previous day is called up in re-representation, and so identifying the negro as 'one of those.' In this interpretation I surely have so far a general idea, and that purely empiric, and which may be considered as the simplest and original form. The practically simultaneous presentation of the individuals as group of very obvious similars—say negroes or oranges—and the use of the same in re-presentation, are the basis of the latter phase where, through reflection and comparison, a number of more or less dischronous presentations recalled are united as group or class, and still later of the self-developing experience in abstract ideation.

Of course group perception in re-presentation is only as yet nascent concept, only general idea in a very crude agglutinative form. The compound group reference is a clumsy and slow method; and so as evolution demands economy the group reference is abbreviated and amalgamated, and the group concept fades into the single image concept, the vague and typical ideal one, representative or vicarious (Wundt) for the many. Thus the idea answering to the word orange

is not a group idea, but a single image so generalized for color, size, etc., that various oranges of different shades and sizes are readily identified under it, whereas in multiple-group-idea the connection is made with the whole group. Hence concept begins with 'oranges' and ends in 'orange.' That by continual reference to the empiric basis as group perceived there tends to come an abbreviation in the number perceived, and a single typical one constructed from the group, is evident if you refer to some experience where you first saw some kind of objects as group, say telephones at an exposition. There you gain in a moment a group perception, which comes up in re-presentation when you afterwards see telephones, but which is continually reduced and made indefinite in later experiences as a datum by which you interpret. Of course many active minds will form the single image concept on the spot, 'anything so and so is a telephone'; but yet the group method may often be traced as evolving into true general idea.

The history of knowledge then is in brief this: the presentation of the individual and recognition by the re-presentation, and practically correlative with this the presentation of masses of individuals and recognition of component members by the re-presentation, which gradually evolves into the vague hypothetical general idea as a shorthand method, which in turn is denoted by language and becomes the basis of all high and self-developing knowledge. The general idea is then a mode of interpretation and as such finite; an omniscient knowledge would know the thing completely without either percept or concept as means, immediate apprehension giving the totality in all its significance. Hence the idea is neither infinite nor eternal. The idea is entirely an empiric help, a mode of reference to the thing through the typical characteristic ideal thing as abstracted from group perception, by which, without the trouble and danger of direct experience, we know the thing for all its qualities for possible experience. And thus as to the distinctive quality of the general idea, we cannot agree with Professor James (*Psychology*, V. 1, p. 468) that this consists in eternal sameness. Self-identity and dogmatism are plainly the inner characteristic of all cognition psychoses, whether of the particular or general, whether mere apprehension or complete interpretation. That the idealist affirms some or all ideas as absolute is an objective value upon which psychology is not called to pronounce.

So, also, it need not be enlarged upon that the distinction of the one and the many is not a peculiar quality of the general idea, nor is that of mere commonness or generality. The dog seeing the man

unclothed and variously clothed undoubtedly forms some general idea of the manifold yet single individual, just as the child does, just as we do. We perceive, equally, the common content quality as equally making the man and men. Thus both percept and concept imply grasp, and the quality of both is the fundamental unitary aspect of all cognition. And so both the individual and the general idea have the same formula:

Anything which has such and such marks is *the* food or *a* food.

This has such marks.

This is *the* food or *a* food.

All cognition as interpretation by sign must assume this form and all consciousness as containing mediating links—one consciousness as leading from a former to a later one—is implicitly rational; and this link as actively sought in object and consciously used in the struggle of existence is reason as either particular or general idea.

If the general idea is an abbreviation of what we have called group perception, then, since group perception is certainly quite early in the history of life, it is probable that the general idea is early also. The dog sees the pieces of meat, his breakfast, as a whole group; and it seems likely that with constant use this *datum* as representation consisting of complex manifold images should be abbreviated to representative single image. While we cannot attain direct test in the case of the dog or other animals, yet the struggle of existence would seem to require that the clumsy and large compound group of images should quite rapidly merge into a single image which should embody the common qualities of all the multiform individuals of the group perception. The analogous stages of perception as leading to particular and general idea may thus be enumerated: 1. The one sense with the one object, the simplest percept, as apprehension of the smooth apple by touch 2. The correlating the senses in immediate apprehension, as in touch, taste, smell, sight of the apple, thus constituting the individual apple by complex psychosis. 3. Direct recognition in representation of the simple and complex individual. 4. Recognition of the essential individuality by the abbreviated re-presentation. Similarly we have for the general idea the four stages, group perception by one sense, by several senses in conjunction, recognition by the complete re-presentation, recognition by the abbreviated re-presentation. It is plain that a new kind of thing, *e. g.*, apple, is as likely first to be given to experience in group as in the single individual, and hence that the phases of development issuing in particular idea and general idea are really parallel, that the general idea is on the same plane as

the particular idea, and not higher, as is commonly thought. The pictures of both individual and group become by the same process highly generalized into an object where certain essential qualities are roughly sketched in abbreviated form, becoming at last a mere notation and sign, of which the evolution of the alphabet from pictographs is both an example and an illustration. The complex simultaneous group perception is certainly more simple and primitive basis for the general idea than the composite photography of objects perceived at different times, which has its place as later and more advanced method. It is plain that the cognition and recognition of the multiple in environment are called for quite early in the struggle of existence. That is, real grasp in perception, in the re-presentation and in its abridgment as general idea may occur among unisensual animals. The paramecium in Mr. Jenning's interesting study (*Am. Journal of Psychology*, x., p. 507) in its feeding contact with 'loose fibrous bodies' may have multiple perception and 'know a good thing when it has it'; and in its continuous feeding, appreciating its food as such, it connects past moment with present by rational interpretation. Again, does not (p. 510) the random *seeking* for food imply ideation as tactile re-presentation? At least the activities of the paramecium suggest awareness, effort and pleasure-pain, if not particular or general representation. But with the dog there certainly seems to be recognition of his master by particular idea, and of men as men by group re-presentation, which probably in such a common case becomes abbreviated into general idea. However, since we have at present no well recognized tests as to consciousness in the lower animals it is unprofitable to discuss the matter, save to remark that the evolutionary doctrine of psychosis points toward an early development of gross general idea as derived from group perception.

However late or early a genesis we find for the general idea, it, on the evolutionary doctrine of the struggle of existence, must like other advantageous life factors originate in severe effortful activity in the critical moment. The one who makes the quickest reference for identification will be the most successful, and at some point in the history of life some organism has, by supreme effort, attained vital advantage by summarizing the group re-presentation in interpreting the present crisis. And if we turn from this theoretical point of view and examine the origin of the ideas in ourselves—the only direct evidence we can have—we find that thought does not arise spontaneously, but the idea has its birth pangs. If we reflect upon our experience in getting the idea of new things, as trolley car or telephone, we perceive that

even in these simple cases the idea is accomplished in self-activity somewhat effortful. And thought is for the vast majority the most laborious and disagreeable of tasks, and with very few who are thoughtful by habit or nature we must refer the origin of habit and nature to integrated effort. We thus cannot agree with those who find the genesis of the general idea in the spontaneous fusion of images into a composite or generic image. And it has not yet been shown how a perception of resemblance results from coalescence of resembling perceptions. This, indeed, gives not even a 'one of those,' but a blurred one which stands merely for and by itself, whose representative value is not understood. However the germ to higher development lies in the simple identification as 'one' (abstraction) 'of those' (generalization). 'That is that,' and 'that is one of those,' give in very abstract language the process of the primitive particular and general idea.

We cannot then agree with Romanes in his doctrine of 'recepts' as the origins of ideas. If we trace the genesis of these recepts we find it in active association, relating or thought. Thus the city man (*Mental Evolution in Man*, p. 50) who in crossing the street and hearing a shout behind him at once has the recept, 'hansom cab,' receives it only because he gained it in early childhood. So while 'obvious' (p. 68) is the law of the recept, we have to trace the obvious as made such by repeated efforts in the struggle of existence. Thus by integration mind as hereditary and habitual function is constituted, and thus the bird has a large obvious which is obscure to man, and *vice versa*.

But if the concept originate in activity we need not, as Wundt, make activity the permanent differentiation of the concept. The thoughts which arise spontaneously in thoughtful people are as truly thoughts in their psychological structure as the same thoughts achieved by most powerful effort by the unthoughtful. Thus, 'This is a picture,' is equally a process of thought whether laboriously attained by the child or enounced as matter of course by the parent. So also for the particular idea, a detective who has the recognizing habit recognizes that face with practical spontaneity, which I, who have no aptitude or custom of remembering faces, recognize only with greatest effort. No evolutionary psychologist can emphasize, as does Wundt, activity-passivity as a mark of genera or species. The genetic psychologist holds the real key to classification of psychoses as progressive functions achieved in the struggle of existence and integrated as 'mind,' just as the genetic biologist holds the real key to the classifi-

cation of living forms as the progressive adaptations to environment which become integrated as 'body.' Hence as *versus* Wundt (Lectures on Human and Animal Psychology, p. 146) and Sully (Human Mind, V. 1, p. 389) we cannot allow that mind ever proceeds as mere apprehension, mere intellectualization, but its development as idea is controlled by interest; pain and hunger incite to grasp in presentation and re-presentation and to the abridgment of re-presentation into general idea.

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ON AFTER-IMAGES—AN EXPLANATION.

In the last number of the *PSYCHOLOGICAL REVIEW* Miss Washburn complains that a statement made by me regarding her experiments on after-images is misleading. This statement was that her subjects were drilled to note images similar to her own.

The denial made by Miss Washburn must be accepted as final evidence that such was not the case, although after re-reading her article in *Mind* I do not find that the account there agrees with her later statement. The following quotation from the account of her experiments will serve to indicate that if the statement made is misleading, it is due largely to the lack of clear expression in her article. She says:

"A reliable test could be had of the degree of special practice attained by the subjects in the course of the research and of the influences of the external sources of error. This test lay in the uniformity of the color changes observed in the ordinary unmodified image. A wholly unpracticed observer noticing the course of an after-image for the first time reports chaotic results and no two observers agree as to the alterations in color which occur. *No results were taken account of from any subject until she was sufficiently practiced to find the color changes approximately uniform*¹ or affected only by such fluctuations as could be accounted for from external causes.

"The first point to be determined was the *sequence of colors to be expected* for the ordinary image under these conditions. *This was ascertained by a series of forty experiments made by W.* at the outset of the research; *the other subjects being then practiced as stated above till their accounts of the course of the image were consistent.*"

Miss Washburn may have intended to say 'till their accounts of the course of the image became constant' (*i. e.*, of less variability). When her last quoted remarks are coupled with the statement that the *normal* or *expected* course of the image was found from the results of a series of experiments made upon herself, a natural inference is that the subjects

¹ These and other words are italicized by the present writer.

were *expected to see* or to get after-images similar to those of the writer of the article (*which were normal*).

Regarding the fact of individual variability, I regret that my results do not agree with Miss Washburn's. In a subsequent portion of the monograph I report that "One subject saw the after-image always as green; to another it was always red; to a third it varied in the white-black series." The differences did not disappear even after the great number of experiments made by my subjects. In addition, it is worthy of note that these differences occurred when the physical light conditions were very constant—probably more constant than the stimulations given by Miss Washburn, for it is well known that the sky is extremely variable not only in intensity but also in quality (color).

The phenomena are so complex that it would be surprising, to me at least, if there was not great individual variability. The problem may repay a more careful and extended study.

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NEWSPAPER SCIENCE.

The newspapers still occasionally publish an alleged statement of mine which was given such wide currency during the summer to the effect that I proposed within a year or two to 'scientifically demonstrate the immortality of the soul.' It is due to the scientific type of mind as well as the correct point of view from which to consider the phenomena for whose explanation I may fairly demand some theory, to say that no such statement was ever made by me either to a reporter or to any one else. On the contrary I was cautious to the extent of saying that I did not pretend to make any such professions, remembering the fakirs that always have promises of this kind on tap. I do not care to disillusion the newspaper public on this point, as my opinion of it leads me to think that it is past redemption. But scientific people sometimes read the papers and are guilty of believing them to the extent of getting the wrong point of view from which to estimate even important facts. It is the correction of that *point de repere* which I wish to establish. Not that I care for any respect that scientific men are capable of bestowing, as there is a good deal which passes for science that is merely respectable, but illusion nevertheless. But what I do wish understood is that, having found it necessary to make some careful experiments in the Piper case, I found it rational to reverse my preferences between materialism and spiritism, though not profess-

ing to demonstrate anything, even to myself, to say nothing of those who have 'the will to disbelieve.' I have deliberately adopted that position in the face of every imaginable suspicion with the intention of leaving it to the unbeliever of anything supernormal at all to rescue me. The time is past when psychology and science can trifle with the subject of spiritism, no matter what the theories that are adopted to explain its real or alleged phenomena. It must either be accepted or killed. Sneering is no longer effective or scientific. Scientific method must control the study of these phenomena or lose its prestige and authority where it has the supreme right, even if it finds it necessary to put illusion and fraud at the very bottom of the universe itself. Personally I do not care what conclusion is reached, especially as in sympathy with Huxley's pessimistic mood I would have a good deal of charity for the desire to boil the human race in Milton's marl of sulphur for awhile and then annihilate it. But this is a matter where wishes avail nothing, and I simply venture to state my facts in due time with the alternative possibilities for their explanation as they present themselves to me, with a preference for that view which keeps science within its chosen field of the finite instead of revelling, like transcendentalism, in an unintelligible infinite. Whether I am right or not I shall leave to others to decide. In the meantime what I shall have to say must not be estimated from the standpoint of fakirs and newspapers. It may or may not have any value, but it does not profess to do any miracles in the way of demonstration.

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PSYCHOLOGICAL LITERATURE.

A Manual of Psychology. By G. F. STOUT. London, W. B. Clive, 1899. The University Tutorial Series. Pp. 643. New York, Hinds & Noble.

Mr. Stout's 'Manual' will at once take rank in the forefront of psychological text-books. It will do so by reason of its admirable readability and clearness of exposition, of its liberal scope of treatment, of its pertinent and practical utilization of daily mental experiences, and of its well-ordered perspective of chapters, as well as by reason of its suggestive originality and its suitability to excite and guide the interest of the student of psychology. These are the qualities that impress the reader whatever may be his special interests or his measure of agreement or disagreement with the general architectural plan of the work or with the particular expression which that plan assumes.

It is fortunately true that the day of independent and antagonistic psychological systems or schools is giving place—has largely done so, in fact—to a recognition of the essential unity of the scope and methods of psychological inquiry; there is approximately, at least, one science of Psychology as there is one science of Physics or Chemistry. The main differences are differences of the attitude or the interests of authors, of the selection of the topics to be emphasized, of adaptation to the particular needs of the public to whom the volume is addressed. A characterization of a new work in these respects will enable the reader to judge of its scope and nature. The common basis of Psychology, its underlying unity, consists in its constant reference to, its dependence upon, its utilization and interpretation of the concrete aggregate of individual reactions of organisms to the environment of which mental life consists. However far away from the starting point our analyses and abstractions may take us, the starting point is at all events well-defined. However it may be with Philosophy, there is no need of bringing Psychology down to earth; to flourish at all it must take root there and expand so far as it has vigor and support, and bear what fruits it may.

The main difference in interest among psychologists is that between a fundamental interest in the mental functions of human and other

organisms, and in the analytical interpretation and explanation of the processes (particularly of the more highly developed and elaborate processes) which these functions inevitably suggest. It is not easy to characterize this difference by an adjective or two. The terms Rational Psychology, Introspective Psychology, Empirical Psychology and others are unfortunate and misleading. To call one 'Physiological Psychology' and the other 'Psychology, Descriptive and Explanatory,' as Professor Ladd has done, is to exaggerate, somewhat, the sharpness of the boundary lines, and perhaps to misplace the lines which mark off the one domain from the other. 'Experimental' and 'Analytic' are another pair of terms that have been called upon to do service for this purpose. Whatever terms may be used they should emphasize the point of view, the directive tendency, the controlling interest, rather than the boundaries of domains of sovereignty of the proposed divisions of Psychology. To the present writer, the term 'Functional Psychology' (in spite of its uncouthness) more aptly than any other, suggests the one interest or sphere of influence; while for the other a term that combines the ideas of analysis and explanation would be appropriate. Each is present in and with the other; foreground and background may shift and be partially interchanged, but the picture is recognizably the same.

Mr. Stout's manual represents the analytic and explanatory interest as uppermost and dominant; but it does so without in any way regarding the other as unnecessary or unimportant. On the contrary the constant dependence of analysis upon observation and experiment is abundantly emphasized by practice if not by precept. Illustrations from animal psychology and from the mental habits of undeveloped races take their place with experiments and observations of introspective human mentality. Nor is such material used merely as illustrative; it serves frequently as the starting point and basis of the analysis which is the main end in view. The selection and perspective of topics reflect this dominant analytic interest. In the discussion of light sensations, theories of color are more fully considered than the phenomena which such a theory might explain; the possible interpretations of the psycho-physic law are more interesting to the author than the facts which underlie the law, or the methods of demonstrating its validity; the same is true of the treatment of spatial perception, of the nervous and motor substrata of conduct and of other important topics. What is thus gained is the concentration of attention on the main problems of psychological analysis; what is lost by the reader is the realization of the steps by which such analysis has

been made probable and may be more completely demonstrated. The sacrifice is, in most cases, justified by the simple fact that it is part of the author's plan and purpose, inasmuch as the scope of his work does not contemplate an adequate consideration of the fact—material and content of psychological lore. One must go to other volumes for such information or for the treatment of the same problems from the 'functional' point of view.

As the work appears in a tutorial series its pedagogical fitness may be properly touched upon. Mr. Stout's position is that sketchiness is above all to be avoided, that the student must 'live himself into psychological problems,' that 'cut and dried statements skimming important questions are of no avail.' These faults the volume avoids and these ends it will accomplish as well as any work that has as yet appeared. For class-room work (which is in a measure opposed to tutorial instruction), systematic statement, and even sketchiness are in part inevitable, in part unobjectionable. But after James no one will hope to satisfy psychological students with anything that is too conventionally cut or shows obvious traces of the drying process. And it is true that the very nature of psychological investigation demands a pliability of attitude, a capacity to observe common occurrences in uncommon ways, which is one of the benefits which a course in psychology should bring about. In this respect Mr. Stout's work is eminently satisfactory.

Mr. Stout further presents an "exposition of Psychology from a genetic point of view. A glance at the table of contents will show that the order followed is that of the successive stages of mental development. The earlier stages have been copiously illustrated by a reference to the mental life of animals. The phases through which the ideal construction of Self and the world has passed are illustrated by reference to the mental condition of the lower races of mankind." This emphasis of the genetic point of view may lead the reader to expect something which the manual does not furnish. There is little in the order of chapters to suggest a specifically genetic treatment as the term is ordinarily understood by students of comparative psychology; what is true is that illustrations of analytic principles and results are drawn from the field of animal minds and primitive human minds with pertinence and effectiveness. The order of chapters, indeed, presents little that is unusual. The introductory chapters present 'the Scope of Psychology,' its 'Data and Methods,' and the general relations of 'Body and Mind.' Book first then considers the general analysis of conscious states, the 'Primary Laws of Mental Processes,' with a sub-

sidiary notice of the theories and weaknesses of 'Faculty Psychology' and 'Associationism.' The next book deals with sensation, taking it up first in its general relation to cognition and experience, to the physiological mechanism upon which it depends, to its interpretation and development and to the 'feeling-tone' which accompanies it; light and sound sensations and the Weber-Fechner law are considered especially in separate chapters while the other senses are briefly, rather too briefly, described. The next division is devoted to Perception and is in many respects the ablest and most valuable part of the work. The treatment falls into two parts: the first or general part considers the nature of perception and the special rôles of imitation, pleasure, pain and the emotions in the development of percepts; the special classes of percepts considered relate to external reality, to the perception of time, and of space as dependent upon touch and sight. The last division takes up concepts and ideas. The treatment is a broad one including chapters on the general relations of ideas to images, on their arrangement in sequences—'trains of ideas,' on their connection with memory and comparison and imagination; on their practical furtherance by language (an interesting chapter) and their significant emotional aspect, on their results in the ideal construction of the world and of self. The last chapter presents a concise treatment of the will in 'voluntary decision.' A chapter which well illustrates the manner in which observations upon animals are utilized in psychological analysis is the general one on perception (Book III., Chapter I.). Chapters VII. and VIII. of Book IV. (on the Self and on Belief and Imagination) as well as Chapter V. (on Language and Conception) furnish the best illustrations of the aid which Analytic Psychology receives from an examination of the mental processes of primitive man. They also illustrate the utilization for the same purposes of the data of mental pathology. But neither this treatment nor these illustrations constitute a genetic point of view as that term is commonly and, it seems, properly understood. A genetic account of sensation would consider the growth and development of sensation from its simplest to its more complex forms; its modifications in successive stages, the sequence of such stages and their dependence upon physiological unfoldment and upon coördinate general psychological processes. Mr. Stout does not consider sensation or other topics in this way; it is not part of the design of the book to treat them in this way. His exposition is quite generally infused with the suggestiveness that comes from a discerning use of genetic data; and that is what must be understood by his 'genetic point of view.'

To indicate Mr. Stout's position on the general problems of Psychology is not a part of the present review. These positions have been indicated by him in his larger and earlier work on 'Analytic Psychology.' The present purpose is to indicate the scope and trend of this excellent tutorial manual to those who consider its use either in the class-room or as collateral reading in connection with a systematic course in psychology. Its use for the former purpose will depend on the nature of the course, the method of instruction and the special interests of the instructor. For the latter purpose its use may be cordially recommended without restriction. As the editor of *Mind*, as the holder of the newly founded readership in mental philosophy at Oxford, as the author of the 'Analytic Psychology' and of this 'Manual,' Mr. Stout's position of leadership and influence in the psychological activity of England is assured; his American colleagues look forward to the fruits of this influence with confidence and with anticipations of aid and inspiration.

JOSEPH JASTROW.

A Theory of Reality. An Essay in Metaphysical System Upon the Basis of Human Cognitive Experience. GEORGE TRUMBULL LADD, Professor of Philosophy in Yale University. New York, Charles Scribner's Sons. 1899.

This volume is a continuation of Professor Ladd's philosophical series, and is the logical outcome of his previous thought as outlined in his *Psychology*, his *Philosophy of Mind* and his *Philosophy of Knowledge*. The *Theory of Reality* is most intimately connected with the *Philosophy of Knowledge*, as is indicated by the sub-title. The author in his introductory chapter contends most emphatically that a systematic metaphysic must be based upon a critical analysis of our cognitive experience, affirming that "the critical theory of knowledge justifies belief in the power of the human mind to know reality and even to give it a measurably consistent, satisfying, and helpful theoretical determination" (p. 27). In his preliminary discussion as to the relation of phenomena to actuality, Professor Ladd holds that the two ideas are not mutually exclusive but complementary and that a phenomenon that is not of and to some real being is inconceivable, while a reality that is not phenomenon to itself or to some other being is unthinkable. Moreover all phenomena must be regarded as standing in some relation to a 'noumenal' self. Every phenomenon then is of some real being—self or thing, and appearing to some cognitive being. This may be regarded as the author's fundamental thesis. In the de-

velopment of this initial position, he proceeds to the analysis of the idea of reality as given in our knowledge of a concrete thing, and he finds that the varied judgments which we form concerning anything in the sphere of reality must be regarded merely as the different expressions of the categories of thought. These categories are not merely thought-categories, but categories of being as well. They are quality, relation, change, time, space and motion, force and causation, quantity and measure, unity and number, form, law and final purpose. There are four characteristics of the categories which are insisted upon as essential to a proper understanding of their nature, and relations. They are, first, "the categories are not separable either in thought, or in reality, as are the concrete realities themselves; second, no single category is recognizable by an analysis of cognitive experience or is statable in thought without involving the recognition and conception of every other; third, no category is completely resolvable into any other; fourth, all the categories form a sort of interior oneness—a system which appears as a harmony to thought and is experienced as effecting a unity in the world of reality" (p. 85). Moreover, the self is to be regarded as the fundamental reality harmonizing in complete unity all the categories in its being and activity. The reality of things is revealed by their relation to the activity of self. "The experience of being checked, and inhibited on every hand is the very core of my cognition of every other thing. My self-felt activity is opposed by that which is not, and can not be recognized by me, as *my* doing. The inhibition is, on the contrary, necessarily recognized as the doing of that which is not me. To that which, not being my self, stands opposed to my self-felt activity I attribute the same essential being which I know myself to have. It, too, is a center of activity which stands to my self-felt activity in the reciprocal relation of acting and being acted upon. It is in this fundamental fact of an activity which is both self-felt and also known to be inhibited that we discover the root, in experience, from which the conception of substance springs forth" (I, p. 123). The reality of things, therefore, is to be conceived after the analogy of the reality of self, and this in many particulars which Professor Ladd dwells upon at some length. For instance, he insists that "to be a real being with actual qualities, is to be what I know myself to be—namely, capable of imitating and of experiencing changes that are attributable to some subject or 'central point of attachment' conceived of after the analogy of a conscious will" (p. 139). And again, "things are known or conceived of as remaining somehow *self*-identical, while being subjects of more or less

important changes, after the analogy of this identity which belongs to the self" (p. 155). These quotations might be multiplied as indicating one of the author's most characteristic positions; indeed, according to his point of view all the categories which function in the consciousness of self operate in an analogous manner in reference to all things which are cognized by self. This analogy is especially emphasized in the categories of form, of law, and of final purpose. And so Professor Ladd insists that "reality in general is known as actually being a unity of force guided by ideas of form and law into processes that conform to ideal ends" (p. 367). He, furthermore, declares that "the forms and laws ascribed by the intellect of man to things have a trans-subjective basis, and are not merely imparted to things by the mental act of knowledge" (p. 347). Finally, the ground of all reality is regarded as the Absolute Being, or the Absolute Self. "We are irresistibly led on from the facts of the interaction of elements under law to the existence of a supreme unity which may serve as a real *locus* for the existence of controlling ideas" (p. 358). All selves and all things have their changing places and functions in the one system because the connection of them all is guaranteed and accomplished by the One Will in its progressive realization of its own Ideas" (p. 362).

These are the salient features, roughly sketched, of Professor Ladd's doctrine. He appears then in the pages of this book as the champion of a trans-subjective reality, conceived after the analogy of that reality which is revealed in the consciousness of self, and which finds its ground and explanation in the all-pervading presence and power of the Absolute Being, the primal reality, and perpetual source of all being whether of a self or of a thing.

In Professor Ladd's general point of view, he has been led to place undue emphasis upon a voluntaristic interpretation of cognitive experience to the neglect of other essential elements which must be reckoned with in laying deep the foundations of a comprehensive and satisfactory epistemological doctrine. In his determination, moreover, of the reality of the not-self, according to the analogy of the self, there is danger of leaving the impression of a crudely anthropomorphic representation of the world and indeed also of the Absolute which underlies the world manifestation.

In finding the essence of reality in the interactions between body and spirit, the author might be forced logically to the position of assuming a like interaction between body and spirit in the being of the One Reality, of which the world, as matter and force, and the vari-

ous selves, as bodies and spirits, are but the partial manifestations. For instance, we read that "the one all-inclusive Being of the World, the Unity of Reality is responsible for the union of body and spirit in each human Self, and of each Self with other selves, and of all selves with all things. If now, however, the language which it has been found necessary to employ in all our explanation of the reality of the self, as dependent for its being and its manifestation upon the Being of the World, is translated over into the thoughts already provided for it, we are led again to the conception of an Absolute Self. For every characteristic of this being of the world, in which all concrete beings 'live and move and have their being,' is constructed after the analogy of the spirit's cognitive and self-active life, in the pursuit of ideal ends" (p. 413). But this 'cognitive and self-active life' of the self, according to Professor Ladd, is a life of joint activity of body and of spirit, and by analogy, the unity of reality would have a being of a like nature.

There is again a danger of losing the self and dissipating its reality by immersing it in the unfathomable deep of the Absolute. Thus, Professor Ladd insists, in the summing up of his argument upon force and causation, that "all the growth of man's cognitive experience reveals the Being of the World as a unity of force, that is constantly distributing itself amongst the different beings of the world so as to bestow on them a temporary quasi-independence, while always keeping them in dependent inter-relations, for the realization of its own immanent ideas" (p. 293). Moreover, Professor Ladd, in attempting a solution of this problem of the relations sustained by the human will to the will of the Absolute, says that "there is not necessarily any more contradiction involved in this so-called 'double aspect' of the relations of man to God than is involved in the consideration of all particular existences from both the scientific and the ultimate, or metaphysical, point of view. Two of H unite with one of O to form the compound H_2O 'because of' the laws of chemical affinity and 'because of' the relations into which the H and O are brought by the compelling forces of their environment, temperature, pressure, induced molecular activities, etc. But the chemical is not the entire explanation of such a transaction in reality. Really H_2 and O come together in this way, because 'it is their nature'; the ultimate explanation takes into account the mysterious being of these elements as a primary postulate, a pre-condition of all the forms and laws of their reciprocal behaviors. Now from philosophy's point of view this is essentially no other than the position H and O behave

in this way because it is the Will of the Absolute that they should so behave" (p. 515). The impression left upon the reader is that the seemingly independent will of personality is in reality none other than the 'quasi-independence' which may be attributed to things. Surely this is a loss of the self as regards the integrity of its free personality. The relation of the Absolute Will to the wills of persons must certainly differ from the relation of the Absolute Will to the manifold forces of Nature.

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Einleitung in die Vergleichende Gehirnphysiologie und Vergleichende Psychologie mit Besonderer Berücksichtigung der Wirbellosen Thiere. By JACQUES LOEB. Leipzig, J. A. Barth, 1899. Pp. 207 and 39 illustrations.

This work from the hands of Professor Jacques Loeb of the Department of Physiology of the University of Chicago is of considerable significance for psychologists. It embodies not only the results of many original researches, but is also a valuable digest of recent literature bearing on the subject. Its importance is enhanced by its negative criticism of current neurological conceptions of cerebral localization and its psychological corollaries. As the author says in the preface, this work grew out of an examination of the assumption made in his previous work¹ that all the reflexes of the animal life are under the specific direction of ganglion cells. This assumption was rendered assailable by the fact discovered by him of the identity in nature of many of the animal and plant tropisms. He was led to one of two conclusions. Either we must attribute consciousness to plant forms, or we must develop a new criterion for the presence of consciousness in lower animals. As we shall see, he adopts the latter alternative.

He takes up first the consideration of the fundamental facts and general principles of comparative brain physiology. The physiological unit has hitherto been regarded as the reflex, while ganglion cells have been regarded as the structural element which makes the reflex possible. Even writers who have seen nothing in the reflex but a mechanically determined activity have regarded ganglion cells as the essential organs of the complicated movements involved therein. Says Professor Loeb, "I never would have doubted the correctness of the older physiology, which gives to the ganglion cell this important rôle, had not my own discovery of the essential identity of the animal and

¹ Especially his 'Der Heliotropismus der Thiere und seine Ubereinstimmung mit dem Heliotropismus der Pflanzen,' Würzburg, 1890.

plant heliotropisms shown the inadequacy of this point of view, and at the same time given me another conception of the real nature of the reflex." This new point of view led him to further experimentation in which these animal heliotropisms are shown to remain even when the central ganglia supposed to be concerned have been removed. In other words, there is a direct relation between the peripheral sense organs and the different musculatures, not mediated by the central nervous system. The general conclusion from his experiments, therefore, is this, that stimulability and conductivity, only, are necessary for reflexes, and that these are universal properties of all protoplasm. Ganglion cells possess no mysterious property, not common to all protoplasmic nervous tissue, for the production of the reflex. The ordinary conception of instincts as inherited reflexes perpetuated by a sort of organic memory in these ganglion cells comes in for its share of criticism. Such a theory can furnish neither a mechanic of instincts nor a natural explanation of their transmission. Their real explanation is found in the phenomena of the various tropisms (heliotropism, chemotropism, geotropism, stereotropism, galvanotropism, etc.). If, then, the mechanics of a number of instincts can be traced back to tropisms common to plants and animals, and if the significance of ganglion cells is limited to the function of conductivity, then we must expect a new statement of the factors which condition these complicated reflexes. Researches on the galvanotropism of animals leads Professor Loeb to conclude that a simple relation exists between the orientation of the nervous elements in the central nervous system (along the line of the chief axis of the body) and the direction of the movement of the body called out through these elements. Tropism reveals itself in two general conditions (1) in the specific irritability or excitability of definite peripheral elements, and (2) in the symmetry of the body. Symmetrical elements of the organism have the same irritability; unsymmetrical elements have different irritabilities. The elements near the oral pole have a greater stimulability than the aboral, or the reverse. The same argument applies to the so-called spontaneous movements, as well as to reflexes. There are two groups of these, (1) simple spontaneous movements, and (2) rhythmical spontaneous movements. That simple spontaneous movements do not require mediation by ganglion cells is apparent, *e. g.*, from the spontaneity of the swarm-spores of algæ. The rhythmical spontaneous movements are more important in this connection. Respiration and heart-beat belong in this category. These are ordinarily attributed to certain ganglia. But the researches of Gaskell, Engel-

mann, Loeb, and others, show that these processes can take place independently of the ganglia to which they have been referred. They do not require a higher coördinating center, but are purely segmental reflexes. This segmental conception is then extended to the whole animal series.

Naturally there arises the question of the psychological application of the foregoing conclusions. It is one of the distinctive problems of brain physiology, in the opinion of Professor Loeb, to develop a theory of consciousness. Accordingly he makes a statement of what he regards as the criterion for the determination of the presence of consciousness in the lower animals. This he finds in what he calls 'associative memory.' By this he means that arrangement through which not only the immediate reactions take place which correspond to the stimulus and the sense organ, but also other effects or reactions which are the normal responses of other stimuli acting either previously or simultaneously on the organism. This had been worked out previously by Professor Loeb in his 'Beiträge zur Gehirnphysiologie der Würmer.' The detailed experimental application of this criterion to all the different classes of animals constitutes the future problem and task of comparative psychology.

On the side of the comparative physiology of the brain the chief object of this work is, thus, to establish the segmental theory of the central nervous system. On the side of comparative psychology, its principal object is to establish 'associate memory' as the criterion for the presence of consciousness in the lower animals. With this general view of his spirit and aim we may pass to a brief survey of some of the facts upon which these conclusions are based.

The first type that we have presented to us is *Medusa*. In those forms of *Medusa* which possess a more highly developed nervous system in the marginal ring (such as *Hydromedusa*) the rhythmic spontaneity is localized exclusively in that part of the animal containing the nerve ring, though in types possessing a less highly developed nervous system (such as *Aurelia aurita*) any portion of the umbrella is capable of exhibiting these contractions. It is fundamental that the continuity of the connections of the nerve ring is requisite for the production of the synchronous movements which are necessary for the contraction of the umbrella. An analogous example is found in the case of the frog's heart, in which it has been found by experimenting on the contractility of different excised portions, that its different parts contract in rhythmic unison with the sinus venosus, which

¹ Pflüger's Archiv. Band 56, 1894.

when isolated, has the greatest rate of contraction. There is no necessity here for a special coördinating center. The stimulation of one part of the organ results in the instantaneous communication of the stimulus to the remaining parts, and the organ practically contracts as a unit. This is confirmed by Dr. Hargitt's researches (under Professor Loeb's direction) on *Gonionemus* in which two individuals were artificially united, and two days after the operation showed perfectly synchronous contractions. In opposition to Romanes, Professor Loeb denies that these contractions are dependent upon the presence of ganglia, either in the case of *Medusa* or in the case of the frog's heart, and this denial is supported by Engelmann's conclusions that the seat of the automatic activities even of the adult heart is the muscle tissue rather than the ganglia. Professor Loeb cites further the case of the Infusoria in which we have rhythmic contractions of the vacuole without the presence of distinct nerve elements. Such reactions as that of the frog in attempting to remove the acid irritant have been attributed to the complex coördinations made possibly by its relatively highly differentiated nervous system. But Professor Loeb shows that entirely analogous reactions may be secured in the case of *Hydromedusa* in which the nerve ring has been excised.

The next researches are on the central nervous system of *Ascidians*. The peculiar 'purposeful' reflexes of *Ciona intestinalis*, ordinarily explained as due to the coördinating functions of its single ganglion, are here shown to remain after its excision. The ganglion has simply the function of conductivity, rendering such reactions quicker, but is not essential to the possibility of such reaction.¹ Similarly, in such reflexes in the higher animals as the contraction of the iris, the function of the central nervous apparatus is demonstrated to be simply the accelerating of the reaction, since contractions take place not only in the decerebrated animal, but in the excised iris. That other tissues beside nervous tissue are capable of such contractions is, again, substantiated by the acknowledged phenomena of contractility under similar conditions in insectivorous plants and in protozoans, where there is no suggestion of a clearly differentiated nervous tissue.

In the *Actinarians* (*Actina equina*) the following experiment was performed. A bit of thread with a piece of meat attached to one end and a bit of paper to the other was laid across its tentacles. In every case, no matter what the relative position of the two ends of the thread, the meat was drawn inwards through the mouth opening while

¹This is reproduced from his previous work entitled 'Untersuchung zur physiologischen Morphologie der Thiere,' II., Würzburg, 1892.

the paper was allowed to remain inertly hanging outside. Here the explanation is similar to that of the movements of carnivorous plants. There is no need for the assumption of the coördinating function of a ganglionic center. The chemical affinity between the constituent elements of the food material and the tentacular structure is sufficient explanation. That there is no purposive consciousness or intelligence here is proved by further investigations of heteromorphosis, in which, for example in *Cerianthus membranaceus*, a new ring of tentacles became artificially developed in connection with a lateral incision. Here the tentacles attempted, as in the case of the true mouth, to force the morsel of the meat into the digestive cavity at the point where the mouth should have been. This occurred as often as the meat was presented to it. There was no suggestion of associative memory, no learning by experience. It was altogether as mechanical as any purely physical or chemical reaction. Indeed, in the case where the new head was developed near the old one, the two sets of tentacles actually entered into a contest each striving to force the morsel of food into its own mouth cavity. In addition to these chemotropic reflexes, the foot of the actinian is shown to be stereotropic, and the general bodily position determined geotropically. An actinian placed in an inverted position in a test-tube, with its head end down, will regain its normal upright position within an hour by the gradual contracting of its body and tentacles. That this is due to the sheer force of gravitation is shown by Professor Loeb's experiments with a horizontally placed coarse screen through which the actinian which was laid upon it slipped slowly into a suspended vertical position in a purely passive way. When the screen was inverted, instead of slipping out, the foot end began to bend and finally slipped through another mesh of the screen, and so on, until the animal lay coiled among the meshes of the screen.

The reactions of *Echinoderms* are shown to belong to the same general category. The ventral surface of the starfish is stereotropic, while in some types there is combined with this a negative geotropism or positive heliotropism which causes these animals to climb up the slant surfaces of rocks or up the vertical walls of the aquarium. It is necessary for the nerve ring to be intact in order that the animal may regain its normal position after having been laid upon its back; but this is not because of any disturbance of a ganglionic center since a single amputated arm will succeed in regaining its original position as well as the normal starfish.

In his researches on *Planarians* Professor Loeb emphasizes in

the first place the absence of any reactions such as in the higher animals are called reactions to pain. If the head end of *Thysanozoon Brocchii* be snipped off as it floats upon the surface of the water, the tail end drops inertly to the bottom, while the head end (containing the only ganglion of its simple nervous system) proceeds on its way unhindered and without any signs of pain from the operation. The peculiar feature about the orientation of these animals is that on the ventral side we have positive stereotropism and on the dorsal side negative stereotropism. Here again the reactions take place quite as well in the absence of the ganglion as when it is present, except that they are slower. If a cross-section be made which is not quite complete, the head end which contains the ganglion will attempt to creep off (spontaneous movements) while the tail end fastens itself to the nearest object (positive stereotropism). In this type the head end only shows the spontaneous movements. But in a fresh water Planarian, *Planaria torva*, though morphologically very closely related, the tail end as well as the head end creeps off in a very lively fashion in spite of the absence of the ganglion. The spontaneity of progressive movement is evidently here again not a function of the 'brain' or cerebral ganglion. The reactions of *Planaria torva* to light are also of especial interest. They take place as well in the decapitated as in the normal animal.

The same question arises in the experiments on *Annelids*. Has the cerebral ganglion any function of coördination in connection with the movements involved in locomotion? This is shown to be not the case. Even in the dog it is shown by Goltz that coördinations for locomotion are explicable in a manner similar to that in worms, *i. e.*, without calling upon the inexplicable functioning of some ganglion. As in *Annelids* the progressive movements of locomotion take place not only in the absence of the cerebral ganglion, but even when the chain of segmental ganglia is itself broken, so in the dog certain locomotor reflexes occur in the hind legs even after cross-section of the spinal cord. In both cases these reactions are due to reflected stimulation through the peripheral organs (skin, muscles, etc.). The difference between the dog and the earthworm, so far as locomotion is concerned, is not so much the difference in the central nervous apparatus as in the development of the peripheral organs. Had the dog, instead of its long lithe limbs, mere stumps instead, we would have the same phenomena and the same explanation as in the progressive movements of the earthworm. Other functions of the earthworm, as in the case of Planarians are explained as due to heliotropism or chemotropism.

The researches on the foregoing forms have taught us, says Professor Loeb, that the characteristic reactions of these animals are determined (1) by the different forms of stimulability and arrangement of the peripheral elements, and (2) by the ordering of the motor organs, the muscles. The central nervous system has no other part to play than that of furnishing a means of quicker and easier conduction, and the fact of the greater complexity of the cerebral ganglion is simply due to the massing of the peripheral sense organs in the head. He then goes on to show how the segmental conception holds also for *Arthropods*. His first example is *Limulus polyphemus*, in which all of the nervous system was extirpated with the exception of a portion of the circumoesophageal ring and the abdominal (respiratory) ganglia. The portions of the nervous system retained enabled the animal to eat and to breathe, and the animal was actually kept alive for some time by food being furnished it. This completely overthrows the conclusions of previous investigators who asserted the existence of a special control by the cerebral ganglion. The relations in higher animals are essentially the same as those in *Limulus*. The usual reference of the coördinating center for the respiratory movements to the medulla, to the point called by Flourens the 'vital node' is altogether erroneous. This view has rested upon the observation that an injury at this point or section of the cord between this point and the point of origin of the Phrenicus leads to the arresting of the respiratory movements. But Professor Loeb shows that this is a temporary inhibition only like that occurring in *Limulus* immediately after the removal of the suboesophageal ganglion. Langendorff,¹ indeed, has made the important discovery that even the decapitated vertebrate, in which the 'vital node' is removed, is capable of independent respiratory movements. In fact, if in the new-born vertebrate the respiration be kept up artificially until the shock effects pass off, the animal will then continue to breathe in a normal manner. To sum up, the common idea, as represented by Faivre's work, that the centers for the reflexes of locomotion, breathing, etc., are located in the suboesophageal or prothoracic ganglia, in arthropods, is wholly rejected, and the conclusion of Bethe adopted, that these reflexes actually have, as they appear to have, their centers, in the corresponding segmental or abdominal ganglia. Or, to state the principle in general terms, each individual segment of a segmental animal may be viewed as a simple reflex animal like the *Ascidian*. The assumption of special higher coördinating centers is wholly superfluous.

¹ Studien über die Innervation der Athmenbewegungen, I., Mittheilung, Archiv für Physiologie, 1880.

The researches on *Mollusks* are wholly in line with what proceeds.

The probability is, then, that the segmental theory holds also for *Vertebrates*. It is Professor Loeb's opinion that the brain represents more, rather than less, segmental ganglia than even investigators sympathetic with this point of view have been inclined to assign to it. Experimentation shows that after recovery from the shock effects of operation, the caudal part of the spinal cord is capable of all the reflexes that are possible when it possesses its normal connection with the cephalic portion (brain). Such reflexes as reaction to irritation of the skin, erection of the penis, secretion of urine, defecation from the rectum and bladder, and the vasomotor reflexes are all retained. Loeb has shown the same to be true in the case of the Crawfish for the respiratory movements, and Goltz has shown the same for the arm reflexes in the frog, while Schrader has shown the same for the movements of locomotion. The cerebral cortex is found to be unnecessary for vision in frogs and fishes: the optic thalamus only is necessary. The theory of a single ruling center or even of a group of cortical controlling centers is thus negatived, and the importance of the independent functioning of the spinal segmental ganglia is emphasized. The chief reasons why the segmental theory has not been accepted hitherto, according to Professor Loeb, are (1) because in vertebrates the brain shows the segmental character only in its earlier embryological stages; consequently only a few brain physiologists have seen that this is the key to the understanding of its function, (2) because the shock effects of operation on the higher animals has so obscured the real effects of extirpation, and (3) because of the influence of a metaphysical psychology with its doctrine of separate functions and corresponding centers.

Professor Loeb's theory of animal instincts grows out of this segmental conception of the nervous system. He looks upon instincts as made up of a series or chain of segmental reflexes (*Kettenreflexe*). By instincts are generally understood unconscious actions of animals directed toward a definite end. Such are the habits of insects which lay their eggs in material which serves later as food for the hatching young. Such also are the periodic migrations of birds and aquatic animals, instincts of self-protection, protection of the young, etc. But many of these so-called 'purposive' instincts are clearly shown by Professor Loeb to be explicable as animal tropisms. His whole chapter on Instincts is very interesting and instructive as throwing light on the segmental theory. Of course, his theory of instincts militates against any theory which suggests that instincts are degraded habits,

the inheritance of fixed modes of action of preceding generations of ancestors. In connection with the question of instincts arises the problem of the transmission of characters. That there is a transmission through the sexual or reproductive cells is not doubted, but the question that has puzzled all investigators is how so simple a structure as a reproductive cell can be the bearer of so complicated an apparatus as that necessary for reflexes and instincts. There are two possibilities. Either the egg is a much more complicated structure than has been supposed, containing all the elements necessary for the fully developed animal, or the complexity of the mechanism of reflexes and instincts can be reduced to the terms of simple elements and processes which can be easily transmitted by the egg without the latter possessing such a highly complicated structure. Weismann represents the former standpoint. The results of Professor Loeb's investigations lead him to adopt the latter alternative. Growth processes are first of all processes of chemical differentiation, with which the particular morphological characters are secondarily associated. The egg is the bearer primarily simply of certain determinate chemical substances, and hence the transmission of characters is possible only in the form of the transmission of these substances, and the nervous system can influence this transmission only as it may be instrumental in changing their nature. Chemical theories of heredity are not new, but in the form in which Professor Loeb presents his theory it has a place quite its own.

This brings us to Professor Loeb's conception of the criterion of consciousness. He finds this, as has been said, in 'associative memory.' Following Mach he identifies what we call self consciousness or the I-consciousness with the memory factor of our experience, and finds the cerebral cortex to be the medium of the associations involved in the more complex reactions of the higher animals. It is no more remarkable that in certain animals we should have a special apparatus for associative memory than that we should have in certain animals a special apparatus for bringing rays of light to a focus for visual perception. In applying this criterion it is found that consciousness is present in most mammals and some lower forms. The dog responds to its name and greets its master joyously, the parrot learns to speak, the dove to return to its cote or to the place of feeding. Even the tree-frog is granted to have consciousness, though the common frog is denied it. To some fishes it is granted, though in the shark its existence seems doubtful. For the presence of associative memory in invertebrates Professor Loeb thinks there is very little evidence. It

may be granted to spiders, certain crabs, and cephalopods; but is denied absolutely to cœlenterates and worms. Especially does Professor Loeb oppose what he calls the anthromorphism of such writers as Romanes, Eimer, Nagel, etc. He finds almost all previous work on ants and bees an overdrawn account of their supposed intelligence, and cites the work of Bethe in support of his own. Many experiments are cited in support of the final conclusion that 'associative memory,' and hence consciousness, is to be regarded as confined to a relatively small part of the animal kingdom. It is not possible to go into the details of these experiments within the limits of this review, but the *facts* seem to be essentially as represented by Professor Loeb. The only query is, whether Professor Loeb's interpretation on the psychological side is the only *explanation of these facts*. Shall we deny a lower form of consciousnesses to these simpler types of animal life simply because we do not find the cerebral apparatus present which conditions our highly differentiated mode of consciousness? May it not be granted that in these lower types associative memory plays a very small part in the life of the animal, and that when it does come it comes in flashes only, in connection with certain exigencies of life, without denying to them either the possibility or the actual presence, at times, of the beginnings of this function? Such researches as those of Professor Loeb no more prove the absence of lower modes of consciousness than the facts of habit in human experience militate against a doctrine of the freedom of the will. A man is no less free because his volitions are regular and rationally systematic. The same is true of the lower animals. Because we have found a statement for the activities of an eel's tail in terms of physics and chemistry, this does not prove that those activities are not also interpretable in another way, provided we can have the chain of psychological inferences unbroken, as in our attribution of consciousness to a fellow human. Are there any greater difficulties connected with the interpretation of consciousness as between man and the dog, than, for example, between the dog and the eel? What we call the voluntary or relatively free acts of the higher type of consciousness, whether it be dog or man, are in no way different psychologically as acts than the tropisms of the lower animal and plant forms, only we have a chemico-physical statement for the latter, whereas we have not as yet, because of the relative incompleteness of our science, an analogous statement for these highly complicated reactions of the higher type of animal. We should be inclined to meet Professor Loeb's encroachments into the realm of the higher animals by not alone granting him what he claims of animal activities (all the invertebrates and

most of the lower vertebrates) for such mechanical or chemico-physical explanation, but we should grant him the whole field, all the activities of the higher animals including man—only, we should insist that after he had covered the whole ground, his explanation of the facts would still be partial only, requiring to be supplemented by what we may call the psychological and teleological interpretation of these same facts. It does not follow, in other words, that there is no conscious side to these phenomena of the activity of the lower animals and plants simply because these activities have been discovered to be mechanically uniform and necessary. On page 162 he cites the facts of the various kinds of tropism in plants and animals as belonging to the same general category as the chemical reaction, say, of the photographic plate, and suggests that since we attribute no consciousness to the latter, there is just as little reason to attribute consciousness to the former. The reply is this: There is not only just as little reason, but there is also just as great reason for attributing consciousness, not alone to the photographic plate, but to all nature as well. This is just the contention of a current idealism in philosophy, that the whole of nature is susceptible of a psychological and teleological interpretation, as well as of a physical or purely mechanical explanation. This whole argument which is employed with such telling effect to prove the utter absence of consciousness may be turned against itself. The error lies, not in the accuracy with which the experimental observations have been made, nor, in general, in the valuable array of facts set forth, but in the *point of view* from which these facts are interpreted, and in the implicit assumption that the physical and mechanical explanation (the 'scientific' explanation, in the narrow sense) is the whole of the matter. There certainly is no reason for assuming the presence of that type of consciousness which is marked by 'associative memory,' in the absence of the cerebral mechanism which is its invariable condition in the higher animals. On the other hand, it is quite as unjustifiable to deny altogether the possibility of any psychological interpretation for these lower forms of activity. Proceeding upon the lines of Professor Loeb's argument, when we have found a physico-chemical statement of the conditions of all human action, 'associative memory' will go out of the same door that has already, in his mind, closed behind consciousness in the lower animals. The steadily increasing indebtedness of biology to physics and chemistry does, indeed, as Professor Loeb contends, cut at the root of unfounded psychological myths such as the theory of a spinal soul, crude pan-psychism, and hasty generalizations

on hylozoism. But this relation of dependency on the physical sciences is to be supplemented by an equally great and increasing dependency, not alone of biology, but also of chemistry and physics, on the psychological point of view. If psychology seems to be losing its position as a distinct science, it is not because the psychological mode of viewing phenomena is decadent; rather, it is because this psychological mode of interpretation is becoming applied to all spheres of activity, supplementing, and, when rightly understood, reinforcing the mechanical or physical interpretation in every department of scientific procedure. Psychology is losing the narrow footing which it once held among the sciences, not to disappear as the superstition or myth of an unscientific age, but to become universalized as the indispensable correlate of all the so-called natural or physical sciences. But after all this has been said, Professor Loeb has certainly rendered a real service, not alone to physiology, but also to psychology, in overhauling in so thorough a manner the unintelligent lumping of all psychical functions in the brain, which has marked so much of the previous work in this field.

H. HEATH BAWDEN.

VISION.

On After-images. SHEPHERD IVORY FRANZ. Monograph Supplement to the PSYCHOLOGICAL REVIEW, No. XII. New York, Macmillan. June, 1898. Pp. iv+61. Price, 75 cents.

The present volume is, in the main, an attempt to deduce the factors influencing the perception of after-images. This empirical portion comprises sections dealing with the threshold, the latent-period, the duration, the fluctuations, the space-relations and the retinal transfer.

Experiments upon four subjects show that to have an after-image appear seventy-five per cent. of the times the eye is stimulated it is necessary to have as a stimulus (*a*) for $\frac{1}{100}$ second and 64 sq. mm. surface of light, an intensity of $\frac{3}{8}$ candle power; or (*b*) for one second exposure, an area of 4 sq. mm. and an intensity of $\frac{3}{8}$ candle power; or (*c*) for one second exposure, an area of 64 sq. mm. and a light of $\frac{1}{100}$ candle power.

The latent period (*i. e.*, the time between the end of the stimulus and the first appearance of the after-image) was found to vary correspondingly with changes in the objective stimulus. The lesser times, the greater areas and lesser intensities seem to make this period short,

whilst opposite conditions of stimulation increase the time. This difference is explained as due to the large image for a large area, to the blinding effect of an intense light and to the movement of the eyes if the stimulus is prolonged. Under the different conditions of the test, the latent period was found to vary from .46 second to 3.4 seconds.

The total and the actual duration of the after-image were found to be dependent in like manner upon the area, the intensity and the duration of stimulation. Changing the intensity from .01 to 1 candle power showed that the after-image remained longer under the second condition. The increase was from 32 to 68 seconds and from 25 to 55 seconds respectively for total and actual durations. Increasing the area from $\frac{1}{4}$ to 4 sq. cm. gave the following results: 28 to 58 seconds for total time; 22 to 44 seconds for actual duration. A change in the duration of the stimulus from 1 second to 10 seconds caused an increase in duration of the after-image from 40 to 68 seconds total duration, 29 to 53 seconds actual duration. The effect of the larger areas is considered to be almost entirely due to the attention, while the greater effect of time and intensity increases may be considered largely physiological. When the retina was stimulated by lights of different colors, but of uniform intensity, the duration of the after-image was not changed. The peripheral portions of the retina seem to have after-images of less duration than what was found for the fovea and adjacent parts. This, it is suggested, may be due to the attention rather than to any special difference in the nervous mechanism. It was found that after-images lasted longer with those individuals who had good eyesight and with those who were considered to be visualizers. 'Bright' boys had after-images of longer duration than did their fellows. Fatigue, imagination, voluntary attention were found to affect the duration of the phenomena. A consideration of the various results indicates that the greater durations are due largely to the attitude of the observer—to his attention—and the physical light-changes have a comparatively minor effect.

Contrary to what was found by other investigators the fluctuations were not found to be constant with any one physical condition. The variation is great and seems to be dependent upon factors other than the changes in the stimulus.

The color-changes and the changes in intensity of the after-image were not constant with any change in the stimulating light, and the great individual differences make the phenomena very puzzling.

The spatial character of after-images is considered briefly. Numerous experiments with naïve subjects show that if suitable stimuli be

presented *and the mind prepared* for the detection of the effect of solidity, the tridimensional character is evident. This would class the appearances partly as perceptual.

The various views regarding the so-called 'retinal transfer' are noted: (1) that the phenomenon is only a form of binocular contrast; (2) that there is a transfer from the stimulated to the unstimulated eye; (3) that the image has a central origin; and (4) that the transfer is only apparent. The numerous facts adduced in support of each view are considered, the results of new experiments are given, and the conclusion drawn that the image has its seat in the stimulated retina, and that the 'transfer' is apparent.

This empirical portion is followed by a descriptive account of after-images, relating them particularly to memory-images, to imagination-images, and to sensations and perceptions. In a concluding section there is given a brief historical résumé of the various theories proposed to explain the phenomena from the time of Aristotle to the present day.

A short bibliography of thirty-one titles is appended.

THE AUTHOR.

Gefühlston und Sättigung der Farben. JONAS COHN. Philos. Stud., XV., 279-286.

In 1894, Cohn maintained, in an article published in the Philos. Stud., X., 562-603, that the pleasantness of colors increases with their saturation, that the more saturated colors are the pleasanter. A few months later, in an article reporting an investigation made in Professor Titchener's laboratory it was said, "we cannot confirm Cohn's rule that the more saturated colors are the pleasanter; individuals differ in this respect." In the recent article, Cohn publishes the results of a set of experiments carried on to account for the different results obtained in the two cases. In the later experiments Cohn used color materials similar to those used in the Cornell laboratory, viz.: the Bradley colored papers. His method was to repeat our experiments according to the serial method as we used it, and to supplement this by the method of comparison by pairs.

Cohn's article may be summed up as follows: The method of absolute individual judgments, as used by Major, is manageable, observers easily become accustomed to the method. Yet, the conditions of judging by the method of comparison by pairs are simpler and, in general, this method is preferable. The regular arrangement of the colors is objectionable since it gives rise to a demand for change. The

order of shades or tints within the same color should vary. If one wishes to determine the influence of saturation one must place together the shades which are nearly alike in brightness.

Results.—The saturated colors are preferred in a majority of cases, yet some persons prefer the less saturated colors. Two suggestions are made in explanation of the rarer class of judgments; it is due either to a varying retention of the original sense feelings, or to the influence of association. Of the two explanations Cohn inclines toward the second. More light could be thrown on the problem by experimenting with a great number of the exceptional type under varying conditions, and by bringing in children.

It seems to the present writer that while Cohn's recent investigations tend to confirm his earlier statement that saturated colors are pleasanter we must still hold that the rule is not universal: there are great individual differences. And, indeed, Cohn admits this in the present article.

D. R. MAJOR.

HEARING.

Über die maximale Geschwindigkeit von Tonfolgen. Von OTTO ABRAHAM und KARL SCHÄFER. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, XX., 6, 408-416.

Über das Abklingen von Tonempfindungen. Von OTTO ABRAHAM. Pp. 417-424.

The authors produced the tones for their investigation by means of a siren—a disc with openings. They got the result that a trill of only two pitches requires a duration of about 35σ of either tone, to be recognized as a trill. When the duration is shorter, not a trill is heard, but a duad of tones. The shortest duration of the tones forming a trill does not depend either upon the absolute pitch or the interval. When instead of a trill a musical figure of four or five notes is used, a duration three times as long as above of each tone is required, in order that the right succession of the pitches may be recognized.

In the second paper Abraham states that the comparatively long duration (35σ) of the tone is conditioned by the auditory *after-sensation*. Alfred Mayer tried to determine the duration of the after-sensation of a sound by the method of interrupting the tone of a tuning fork by periodically closing and opening a pipe, through which the tone is heard. No doubt the duration of the after-sensation cannot be determined by this method. But the incorrectness of the method

cannot be proved by Abraham's argument. This is based on the assumption that higher tones of an equal intensity of stimulus have a greater intensity of sensation than lower ones. I have claimed in the *Zeitschrift* (Vol. XX., p. 18) that this assumption is by no means self-evident, but must be proved. The mere appeal to authorities such as Helmholtz and König does not prove anything. To prove his assumption, Abraham had to give an exact definition of 'intensity of stimulus.' This he defines as (the variation of) 'the pressure of the air, *i. e.*, the amplitude of the particles of air,' as if 'amplitude' and 'pressure' were identical. Of course, the variation of pressure being represented by a curve, the *amplitude of the curve* would be equivalent to the variation of pressure. But the *amplitude of the particles of air* is something quite different.

Abraham further alleges that the amplitude of the membrane of the Edison phonograph is proportional to the amplitude of the tone. But what is meant here by 'amplitude of the tone'!—amplitude of the particles of air or variation of pressure? Probably the latter, because the amplitude of the membrane, without regard of the frequency of vibration, is proportional to the variation of the pressure, but not to the amplitude of the particles of air. Now Abraham concludes: If the velocity of the cylinder is changed, only the pitch of the tone is changed, but the variation of the pressure of air remains unchanged, because the amplitude of the membrane is unchanged. That this conclusion is wrong, any physicist who knows the elements of mechanics could have told him. Although the amplitude of the membrane, when forced into vibrations by the shifting pressure of the air, without regard of the frequency is proportional to the variation of pressure; the variation of pressure, when caused by vibration of the membrane, is *not* independent of the frequency of vibration. The former is the case because the natural tone of the membrane of the phonograph is considerably higher than the usual musical tones; the latter, because the velocity of the propagation of air waves is greater than the maximum velocity of the vibrating membrane. Consequently the higher tones produced by the phonograph were stronger than the lower ones, because the stimuli were stronger. The same is true of the experiments of Helmholtz and König, quoted by Abraham.

The real argument against Mayer's method is quite a different one, which Abraham has totally overlooked. When—as in Mayer's experiments—a tone of 64 vibrations is interrupted 25 times, each short stimulus consists only of 1.3 vibrations, or if we regard the fact that the tone, when theoretically kept from the ear, practically acts upon

it with a small intensity, each stimulus consists of 2.6 vibrations. But since 2 vibrations at least are required in order to produce a sensation of tone, how can 2.6 vibrations (although varying somewhat in intensity) cause the sensation of tone to rise and fall in intensity. For this effect naturally at least 3 vibrations are required. So it is a matter of course that the tones in Mayer's experiments at last became continuous; but no duration of an after-sensation was determined.

Abraham is in so far right, as he states, that, to determine the duration of the after-sensation of a sound, two tones of different pitches must be used, and not only one periodically interrupted. What Abraham and Schäfer have determined, is, in any case, not the whole duration of the sensation, but by far the greater part of it. So we notice the interesting fact that the shortest duration of a certain sensation (over 35σ) is about 60 times as long as the duration of the shortest stimulus (0.6σ), by which the sensation is produced.

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Die Bestimmung der unteren Hörgrenze. Von KARL L. SCHÄFER
Zeitschrift für Psychologie und Physiologie der Sinnesorgane, Vol.
XXI., 3-4, 161-173.

The lowest tones, as other tones, may be produced by vibrations of the air of a corresponding frequency, or they may be difference tones or tones of intermittence. Schäfer used the two last methods in order to determine the lowest tones audible. These methods have the advantage that overtones cannot make the result questionable, because difference tones and tones of intermittence are simple. The lowest difference tone he heard with certainty corresponded to 27 vibrations. In one case he thought he heard a difference tone corresponding to 14 vibrations.

I may add that, when I made the same experiments (not published) some years ago, the lowest difference tone I heard corresponded to 17 vibrations. [The lowest tones of intermittence that Schäfer heard while using the third of the methods above mentioned corresponded to 16 vibrations.] The possibility of hearing such low tones depends greatly on the intensity of the stimulus.

MAX MEYER.

TASTE.

Geschmack und Chemismus. Von WILHELM STERNBERG. *Zeitschrift für Psychologie und Physiologie der Sinnesorgane* 20, 385-407, 1899. Also *Archiv für Anatomie und Physiologie (Physiologische Abteilung)*, 1899. 367-371.

The author holds that there are only two kinds of taste sensations—sweet and bitter—and that all other forms of taste are ‘heterogeneous sensations of touch or combinations of such with taste.’ Disregarding for the present the intensity of the sensations produced, he seeks to answer the following questions from a consideration of the composition of various chemical compounds and the taste they produce: (1) Why do certain substances possess taste and why are others that are not less soluble tasteless? (2) Why do some substances taste sweet and others bitter? (3) The psychophysical question: Why is sweet agreeable and bitter disagreeable?

Briefly stated, the general conclusion reached is that taste-producing substances have a ‘double nature,’ and that when the particular saporific groups contained in the molecule are arranged ‘harmoniously’ with regard to their number sweet taste results, when this ‘harmony’ is lacking bitter taste is produced.

In the case of organic substances the saporific groups are considered to be OH and NH_2 . The former must be in combination with an alcoholic radicle and the latter with a carboxyl group; but “in order that sweet taste may result, the alcoholic radicle and the OH group must be harmoniously linked with respect to their number.” Again, the amido and the carboxyl groups must be in *α* or *ο* position with respect to each other to preserve this ‘harmony’ and so produce a sweet taste. As special examples for illustration are cited the following facts: polyatomic alcohols are sweet. If in them methyl or more hydroxyl groups are introduced they still remain sweet, but the introduction of phenyl results in the formation of a bitter compound. The introduction of carbonyl or aldehyde groups in the polyatomic alcohols, forming sugars, increases the sweet taste. But if in the sugars phenyl is introduced bitter substances result; thus the glucosides which are largely phenyl derivatives of glucose are bitter. Resorcinone and hydroquinone are sweet, and pyrocatechin and pyrogalllic acid are bitter. The amido acids are sweet as are also dulcine, the condensation product of urea and phenetol, and especially saccharine. Quinine is the bitterest substance, but by introducing ethyl carbonic ester into its molecule a tasteless compound results.

The inorganic compounds of those elements that are in the middle of the periodic or natural system and that have neither strongly pronounced positive nor negative characteristics are sweet, and compounds of the elements of the other groups are bitter.

The paper contains no data of special experiments. The author bases his theory—if it may be so termed—upon a collection of facts largely well known. Many of the additional allusions, etymological explanations, etc., that the article contains may be interesting, but they are hardly to the point and do not serve to strengthen the author's theory.

As far as the 'double nature' is concerned upon which, according to the author, saporific power depends, it may with propriety be held that all chemical compounds (except the molecules of the elements themselves) have a dual nature, since, speaking in general terms, it is *unlike* elements that combine to form chemical compounds. Again the author explains only in a vague, indefinite way what he means by the 'harmony' in the compound upon the existence of which sweet taste depends. By his arbitrary assumption that there are only two taste sensations, he eludes the insurmountable difficulties of explaining the taste of salty, sour and alkaline substances by means of his theory.

Though the article under consideration is interesting to read, the reviewer must confess that in his humble judgment it has not answered in any satisfactory manner the three important questions that it has raised.

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TOUCH.

Ueber die Function der Tastkörperschen. M. VON FREY and F. KIESOW. *Zeitsch. f. Psych. u. Phys.*, XX., p. 126.

This is a very careful investigation of the stimulus to touch sensation. The following facts "necessitate the assumption that, as in the field of the other senses, so also for the sense of touch, the external stimulus acts only as a liberator of energy (auslösend); that the energy in the nerve-fibers peculiar to the excitation-process arises, not from the activity of the stimulant, but from chemical transformations in the end-organ, of which the stimulant is merely the occasion."

1. The depression-energy necessary to excite the peripheral nerves is several hundred times greater than that of the weakest touch-

stimulus. 2. Continuous pressure stimulates the touch-organs but not the peripheral nerves. 3. After a heavy and not too brief weight upon the skin, the sensation continues after the outer stimulus has been removed.

The authors investigate this process of liberating energy (*auslösungsvorgang*), the factors involved being: (1) the locus of the stimulation, (2) its quantitative aspect, (3) the depth of the depression and (4) its time-aspect. They eliminate the last factor, wherever the surface stimulated remains constant, by keeping the time of the depression-process constant. Where the surface stimulated varied, the time was varied according to a rule resulting from experiments previously carried out by Dr. Kiesow. The rule is as follows: with constant pressure, increase in the surface area stimulated necessitated a slow proportional increase in the rapidity of the depression: decrease of the surface-area necessitated a rapid proportional increase in rapidity of depression.

As to the locus of stimulation. Areas differ: (1) in the sensitive-ness of their end-organs, and (2) in the number of their end-organs. Places free from hair must be sought out, as using the razor does not remove the difficulties which the hair-cells involve. The problem demanded that the experiments be carried out by stimulating single touch-organs, and this is possible only on such portions of skin as do not contain too many touch-organs. Tendons and blood-vessels, concave and convex surfaces had to be avoided. The place chosen was the volar side of the wrist. After carefully searching out the loci of the touch-organs, the area experimented upon was drawn upon a chart, each point being carefully marked and described.

After the time and loci of stimulation had thus been arranged for, the author's simplified problem related to the significance: (1) of the surface area, and (2) of the depth of the impression in the stimulation of single anatomical elements. As, however, the depth of the depression was empirically adjusted to the varying anatomical characteristics of the skin (the authors find an empirical adjustment the only possible one), the main question investigated was the significance of the quantity of stimulated surface to touch-sensation.

The apparatus for applying stimuli was but slightly different from the hydrostatic threshold-scales described by Dr. Kiesow in a previous report. Metal pieces were carefully cut to correspond to the size of single elements or touch-organs beneath the surface of the skin, and to various combinations of single elements. For single end-organs the optimum demanded was 0.4 mm.³ or a circle of 0.5 mm. diameter.

Stimulations covering 0.05 mm.²—0.005 mm.² were affected by the use of hairs. The disturbing effects of weariness, varying conceptions of the nature of the experiments in the mind of the subject, variations of temperature in the room, etc., were carefully eliminated. The two authors experimented upon each other as subjects.

The main results of the investigation are as follows: for the stimulation of any particular touch-organ the production of a certain depression with constant time and depth characteristics at the point of skin beneath which the organ is located is the necessary condition. The excitation of a touch-organ is a function of the depression necessary at its locus. The barely noticeable depression increases slowly but clearly when the surface area is increased; and this generalization holds for large as well as for small stimulated surfaces. Where the time is constant, such stimuli as produce a like depression possess equivalent stimulation-values.

One wishes that these careful and elaborate experiments had been carried out on a larger number of subjects, and that central influences such as attention, preconception and suggestion had been investigated. The question arises whether the Druckgefälle of a particular touch-organ, here treated as a constant, does not vary with central influences which the authors do not take into account.

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G. A. TAWNEY.

FEELING.

Zur Kritik der Wundt'schen Gefühlslehre. E. B. TITCHENER.
Zeitschrift für Psych. u. Phys. d. Sinnesorgane, Bd. XIX. Pp.
321-326. 1899.

Bemerkungen zur Theorie der Gefühle. W. WUNDT. Philoso-
phische Studien, Bd. XV., Hft. 2. Pp. 149-182. 1899.

The more often an expert opinion is expressed, the more liable is it to suffer variation and possible inconsistency. This danger is more pronounced when the expressions are offered in independence of one another. Variations are sometimes due, no doubt, to a progress in the views of the expert, as well as to the modifying influence of newly discovered facts. Inconsistencies may arise through forgetfulness or simple logical weakness. The literature under consideration is of interest because such tendencies are more or less apparent in it. Wundt has given three definite expressions of his psychological views¹

¹ *Grundsätze*, etc., 4th ed., 1893, I.: 555-600; *Lectures on H. and A. Psych.* (Eng. tr. based on 2d German ed.), 1896, pp. 210-222, 247f.; *Outlines of Psych.* (Eng. tr.), 1897, pp. v-vi, 74-89. Cf., also Titchener, *An Outline of Psych.*, 1896, Ch. V., especially sec. 34, pp. 105-108.

(nine, if we count the several editions of the works and omit reference to the articles appearing in the *Studien* and elsewhere), which, in so far as they relate to the nature of feeling, seem to Titchener to be at variance, both among themselves and with the observable facts in the given set of phenomena.

The sole aim of the writer in I. is to bring forward a few objections against Wundt's theory of the attributes of simple feelings. These criticisms must be derived from introspection and general reasoning, chiefly because, as it is alleged, Wundt has defended his views on no other grounds, and, in part, because of the lack of experimental data in this field. Wundt distinguishes three chief 'directions' of the primary qualities of feeling, designated by the following pairs of terms, which indicate their opposite extremes: 'pleasurable' and 'unpleasurable,' 'arousing' and 'subduing,' 'strain' and 'relaxation.' (*Outlines*, pp. 82, 83.) Each one of these principal directions is supposed to contain an unlimited number of variable but nameless qualities.

The critic brings forward the following objections against the theory: 1. The logical arrangement of the effective contrasts is in error by implying negation. Pain is not the absence of pleasure. The classification is an attempt to bring together two entirely different things, viz., adherence to the logical scheme of feeling-opposite feeling, and faithfulness to reports of introspective observation. The second and third contrasts are, however, less apparent in immediate experience than the first contrast. 2. In the *Lectures*, pleasure-pain is presented as the preëminent qualitative characteristic of the entire content of consciousness, while the two remaining contrasts are connected with the intensity and the time-rate, respectively, of sensations and ideas. This is merely an arbitrary multiplication of the chief directions of the feelings. The spirit of Wundt's classification is culpable for leaving out the *spatial* aspects. If we have a world of temporal relations, one of intensities, and one of qualities in our life of feeling, then we must also allow a world of spatial relations. The critic suggests the direction of 'expansion-contraction.' This soon betrays itself as an invented process, which speaks against it, and also against the whole classification within which it would have a place logically. In the *Outlines*, the directions are as stated above; the first contrast referring to the momentary state of consciousness, the second to the succeeding states, the third to the determinations which carry over from preceding states. The reconciliation of these two expositions is a stumbling block to the critic. That contained in the

Outlines is in agreement with Wundt's general theory, but is the less probable, and a daring hypothesis which is unsupported, except, perchance, by facts not yet discovered. 3. Wundt's theory is negatived by an alleged discovery of those pertinent facts, gained by the introspection of a student of psychology at Cornell during 1897-8. This student, knowing Wundt's theory well, brought to light the result that at no time during the year of observation was there an emotional content, excepting pleasure-pain, which he could not locate exactly in some of his bodily organs, *i. e.*, every feeling was discriminated as a sensation-complex. Thus the patient thinking of the expert is brought to zero with the greatest dispatch, in a presumed hope of working out a statement of a problem that may be treated only by physiological and experimental methods!

In II., Wundt replies, with his usual leisure, in terms of fact and in terms of logic. 1. The objections of the critic are based upon an erroneous idea of the reasons which led to the formulation of the three-dimensional scheme. 2. The exposition of the several views is not free from errors and misunderstanding. 3. The test which the critic applied to the theory in the way of observation, gives rise to considerable doubt.

These points are specifically worked out in such a manner as to show that the character of the treatment in the *Lectures* and *Outlines* ought to have held back the criticism that the scheme had no other ground than mere reasoning. The critic omits, in a remarkable way, all recognition of the experimental observations used in the first exposition (*Grundzüge*, I., 563ff.). The first part of the reply gives a selective review of the results of Lehmann's plethysmographical study of the bodily expressions, presenting six curves which are almost entirely corroborative of the theoretical analysis in the *Outlines*, which was based, in part, on the studies of Mentz, Mosso, Kiesow and others. Wundt insists that "any distinction of various dimensions of feeling is 'up in the air' so long as its foundation is not placed directly in the subjective observation of the feelings," the real basis of his own scheme (p. 165). Wundt again admits that there are certain *Lücken* in his analysis of the composite forms of emotion, which can be filled up by further experimental investigation. He also finds suggestive corroboration of his fundamental analysis of feeling in Vogt's inquiries into hypnotic states of consciousness. With reference to the logical reductions by the critic, the reply runs, that etymology cannot serve the problems of psychology, nor can there be any further argument where one party does not find, by introspection, any positive

state known as 'relaxation,' etc. Spatial relations did not appear in the original schemes simply because (not alone of their unintelligibility in this connection) they do not appear in either immediate observation or the expressive movements. The apagogical method of the critic involves a complete denial of any quality, or direction whatsoever, as belonging to simple feeling. The reply also finds the accidental introspection of the student to instance no improvement upon the old method of psychologists, for a failure to note which the critic cannot be excused. The sweeping conclusions drawn from the data gained by this method are untenable, being supported by the mere logical supposition of an opposition of sensation and feeling.

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EDUCATIONAL PSYCHOLOGY.

The Physical Nature of the Child, and How to Study it. By STUART H. ROWE. New York, The Macmillan Co. 1899. Pp. xiv+207. Price, \$1.00.

This book is an excellent manual of the simpler tests of the senses, motor ability and bodily conditions, written by one actively engaged in school work and intended in the first instance for teachers. Of its scope the author says in his introduction: "The design of this book is not to give the latest results in each department of child-study touching its physical side, but to give teachers practical hints which may call attention to some physical peculiarities of children, not commonly investigated in their bearing on the work of the schools." The tests are such as can be easily made without special apparatus and in the case of possible ailments of a serious nature are intended only to select pupils to be submitted to expert examination at the hands of a physician.

The first few chapters deal with sense tests; these are followed by those on motor ability, enunciation, nervousness, fatigue, diseases, habits of posture and of movement, growth and adolescence, and school and home conditions affecting the child's physical nature.

The work closes with a bibliography of about a hundred titles and a full index. A few minor errors may be noted, as, for example, on p. 13, where the statement about the width of the small letter *x* is obviously intended for the width of the lines of the letter, on p. 27, where the acoumeter is described as an instrument arranged to tap on *wood*, and on pp. 20 and 21 where the description of the tests for color-blindness, if not absolutely misleading, could be decidedly improved. The

book as a whole, however, is practically unique in its field and admirably suited to its purposes.

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Psychologie mit Anwendung auf Erziehung und Schulpraxis.

DR. KARL HEILMAN. Leipzig, Verlag der Dürr'schen Buchhandlung. Pp. 86.

This third edition of this small work appears after seven reprints of the second edition. It treats of the life of cognition, the life of feeling and the life of volition, embracing 32 sections in all. Illustrations and drawings are used. The elements and essentials of each topic are followed in each instance by a paragraph of application to education and instruction. The psychology is not only elementary, in many particulars it is rather old; but the essentials of each discussion are outlined in simple and compact form. The pedagogical applications made by the author are unexpectedly praiseworthy for so brief an exposition. As in the psychological section, so also in the pedagogical section, one finds only the alphabet of the subject, but these rudiments are very forcibly and helpfully presented. The book is not a pseudo-scientific popularization; it is, on the other hand, filled with fundamental truths and suggestive observations gathered from both experience and study.

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ETHICS.

A System of Ethics. By FRIEDRICH PAULSEN. Edited and Translated from the Fourth Revised and Enlarged Edition. By FRANK THILLY. New York, Charles Scribner's Sons, 1899. Pp. XVIII., 723.

The English reader will feel himself under a burden of obligation to Dr. Thilly for his painstaking and very readable translation of Professor Paulsen's masterly treatise on ethics. Although not written for the delectation of philosophers, who as the Professor somewhat cynically remarks, 'are already overburdened with ideas,' the book is one which the philosopher may read with profit; while to the intelligent lay public that is interested in practical questions, it will furnish a rich and varied treat. To readers of this class the somewhat loose and unconventional style will be a recommendation, while the good sense, catholic spirit and full acquaintance of the author with his

theme, will carry conviction where a more labored and technical argument would fail. The translation covers the first three parts of Professor Paulsen's work, leaving untranslated Book IV. on 'The Forms of Social Life,' the first three books treating of the history of ethical conceptions, the fundamental concepts and principles of ethics, and the doctrines of virtues and duties.

The historical review is very thorough and evinces the author's extensive acquaintance with the development of ethical thought. His opinion is that the Greeks have long ago discovered the fundamentals of moral science and his aim in the history is to show how the Greek conceptions, temporarily displaced by the introduction of Christianity, gradually reasserted themselves, though in a more or less profoundly modified form, as the shaping ethical norms of modern thought, while, therefore, Professor Paulsen does not assign the supreme place to Christianity in the ethical movement which some would claim for it, he treats it on the whole with sympathetic insight. Modern ethical thought beginning with Hobbes and marked in its beginning by a strong reaction against theological ethics has developed mainly along two characteristic lines, what Professor Paulsen calls formalistic intuitionism on the one hand and teleological hedonism or Utilitarianism on the other, Kant being the great exponent of the former, while the English utilitarians from Locke to Mill represent the latter.

In Book II. Professor Paulsen defines his own theory as *Teleological Energism*, thus putting himself in line with the best Greek tradition and especially with the theory of Aristotle. Ethics is teleological in the sense that its central category is that of the good, while it is energistic rather than hedonistic, inasmuch as the direct end is not happiness or pleasure, but perfection of the life activity. On this basis Professor Paulsen develops his own doctrine of the leading ethical exceptions and in connection with it an important criticism of opposing views. It would be impossible to follow him through the discussions of such topics as the highest good, pessimism, duty and conscience, egoism and altruism, the relation of morality and religion, the freedom of the will and self-control. Professor Paulsen is largely in sympathy with the doctrine of evolution and his tendency is to seek in the stream of social and political development for the growing content of morality. The moral is the national or race consciousness asserting itself in the conscious life of individuals and its content develops as a body of customs (*sitten*) which take the form of rules, that impose themselves as objectively binding. On the subject of the relation of religion to morality, Professor Paulsen mediates between two opposing

views. While refusing to allow that morality rests ultimately on religion, he claims for religion a very important historical function in the development of morality and he is also of the opinion that religion is a very important agent in keeping morality alive and vigorous.

Book III. treats of the leading virtues and duties under such topics as the bodily and economic life, spiritual life and culture, honor and love of honor, compassion and benevolence, justice, love of neighbor and veracity. It is in this sphere of applications that Professor Paulsen's peculiar endowment of discriminating good sense shows itself most clearly. One cannot follow his perfectly candid and sincere discussions without getting a great deal of help in the solution of his own practical problems. In fact, the principal value of the book consists in the practical wisdom which illumines almost every page, and it will be likely to be most helpful to the one who comes to it not for an authoritative enunciation of principles but for helpful suggestions in the treatment of practical moral issues.

One may be disposed to admit the general tenability of Professor Paulsen's doctrine without agreeing with him on special points. Doubtless, both the hedonist and the intuitionist could defend themselves against his criticisms, and the special student could, no doubt, urge good reasons in favor of a more rigid and technical treatment. For ourselves we prefer to take Professor Paulsen as he is and professes to be, not as a philosopher's man who is aiming at a strict formulation of principles, but as one who prefers to take the intelligent lay reader into his confidence and discuss with him in an unconventional way the practical questions of his every day life.

A. T. ORMOND.

PHILOSOPHY, PSYCHOLOGY AND PHYSICS.

Address to the Mathematical and Physical Section of the British Association for the Advancement of Science at the Dover Meeting, September, 1899. J. H. POYNTING, F.R.S. *Science*, N. S., Vol. X., pp. 385-396; *Nature*, Vol. 60, pp. 470-474.

There is evident in many places a movement tending to unite philosophy with science after a century of partial separation. Psychology, which has become a science without definitely breaking off from philosophy, has played a somewhat important part in this movement, and the psychologist may at least claim a maieutic function in a rebirth of the utmost importance for the advancement of knowledge.

Professor Poynting's address at the recent meeting of the British Association is a good example of the *rapprochement* referred to, and is especially interesting because he feels justified in claiming that he represents a point of view in which physicists are tending to agree.

For Professor Poynting 'causes' should be abolished in physical science, being appropriate only on the psychical side; 'laws' do not prescribe the course of nature, but are descriptions more or less exact; heat may be more real than kinetic energy, hypotheses, such as those of atoms and of the ether, are not on an equal footing with phenomena observed by the senses, but are temporary scaffolding, ejective rather than objective. Professor Poynting's address is so clear and concise and at the same time so accessible that an abstract is unnecessary, and I shall confine this notice to two points open to question.

Professor Poynting says: "The special molecular and ethereal machinery which we have designed, and which we now generally use, has been designed because our most highly developed sense is our sense of sight." Now we undoubtedly do, most of us, live chiefly in a visual world, but I believe that our world of physical science has been chiefly derived from the kinæsthetic senses. In the visual world the same space varies in size, parallel lines converge, uniform motion may be any kind of motion, etc. It is probably the senses connected with touch and movement that have led to the assumption that matter and motion are real, while color, etc., are secondary qualities, and also to the anthropomorphic ideas of laws and causes.

In the second place Professor Poynting's claim that mechanical description applies to non-living, but not to living matter appears to me invalid. If the movements of an animal are teleological, none the less so is the distribution of light, heat, moisture, soil, air, etc., which makes these movements possible. Certainly we can prepare a Nautical Almanac years in advance, while we cannot surely predict the next movements of an *amœba*. But the time element is not essential; the solar system may simply change more slowly. We cannot predict what solar system will arise from a new nebula. Perhaps we can describe and predict the motions of the solar system, because we need to concern ourselves with only one solar system which happens to be quite a simple and permanent affair; whereas there are innumerable living things each mechanically far more complex than the solar system.

J. McK. C.

NEUROLOGY.

Sur le siege des images motrices. DR. PAUL HARTENBERG. *Revue de Psychologie*, April, 1899. Pp. 109-115.

This article (by one of the editors of the new magazine whose alleged purpose it is to afford physicians the benefits of modern psychology) discusses briefly the 'motor image' from the modern or physiological view-point.

An image in the sense here employed is, "before all, a special nervous mechanism, or the harmonious activity of a group of nervous elements, or finally, a functional synthesis of neurons"; a motor image is then, in particular, the neural mechanism which presides at the performance of a movement, and especially the cortical part of this process. This motor image is to be clearly discriminated both from an image of a movement and from the 'cortical representation of a movement.' The term motor image, Dr. Hartenberg thinks, should be applied strictly to the *cortical* motor mechanism or process. In articulating a word, for example, there is activity in three neural centers: in the bulbar centers of execution (motor roots of the pneumogastric, hypoglossal, spinal accessory and facial nerves); cortical centers for projection (the common motor zones for movement of the lips, tongue, larynx, respiratory apparatus, etc.); and a cortical center of association (Broca's convolution, foot of the left third frontal). To the last of these alone the term motor image should be applied, thinks the writer, for it is here that the essential process of executing a movement takes place, here alone the elementary impulses being combinable; here it is that they are coördinated and really made into a movement.

The place of this process in general seems to be on the confines of the area of the association center of Flechsig, on the marginal zones; as yet only the centers of articulation and of writing are exactly located.

In conclusion, the author considers any value which the neuron theory may have to assist understanding of cerebral activity. Psychological activity, he thinks, is infinitely more complex than the simple physical extension and retraction of parts of a cell: "these phenomena, admitting that they are true, aid one's understanding perhaps, but they explain nothing, for they must themselves be explained. * * * We have as yet not discovered, in the microscope, the secret of human thought."

GEORGE V. N. DEARBORN.

Ein neues Algesimeter. PROFESSOR DR. W. V. BECHTEREW.
Neurologische Centralblatt, 1 May, 1899, No. 9. Pp. 386-390.

This new algometer devised by Professor Bechterew, of St. Petersburg, will have certain value in the laboratory of normal psychology, and will prove of considerable importance, probably, in researches on abnormal subjects, especially in 'nervous clinics.' The first part of the article describing it is devoted to a discussion of the merits and defects of like instruments already in use.

The new algometer consists essentially of a brass cylinder and a flat disk at the end about 1.5 cm. in diameter. The center of this plate is pierced, and though it extends on occasion a sharp steel point, released by pressure on a trigger and extended by force of a spring. The degree of its extension beyond the disc is regulated by turning the latter, the degrees being marked around the cylinder. Thus the instrument is useful in all degrees of paralgnesia up to analgesia. Several forms of point and surface can be screwed to the handle in place of the one described, and besides the plain handle, a dynamometer with a circular dial reading up to 500 grms. is furnished to which as a handle the end pieces already mentioned may be screwed; this dynamometer adds very largely to the general usefulness and desirability of the instrument. Richter, of St. Petersburg, makes the set and sells it at 25 roubles.

GEORGE V. N. DEARBORN.

NEW BOOKS.

Nouvelles reserches sur l'esthétique et la morale. J. P. DURAND.
Paris, Alcan, 1900. Pp. 275. 5 fr.

Science sociale et démocratie. G. L. DUPRAT. Paris, V. Giard and E. Brière, 1900. Pp. 320.

La doctrine de Spinoza exposée et commentée a la lumière des faits scientifiques. EMILE FERRIÈRE. Paris, Alcan, 1899. Pp. x + 357. 3 fr. 50.

Vues contemporaines de sociologie et de morale sociale. HENRY LAGRÉSILLE. Paris, V. Giard and E. Brière, 1899. Pp. iii + 268. 5 fr.

Les philosophies négatives. ERNEST NAVILLE. Paris, Alcan, 1900. Pp. 263. 5 fr.

La philosophie naturelle. W. NICATI. Paris, V. Giard and E. Brière, 1900. Pp. xi + 308. 3 fr. 50.

The Theatetus of Plato, A Translation with an Introduction. S. W. DYDE. Glasgow, James Maclehose and Sons, 1899. Pp. viii + 173.

Crime and Criminals. J. SANDERSON CHRISTISON. Second Edition. J. S. Christison, Chicago, 1899. Pp. 177. \$1.25.

Brain in relation to Mind. J. SANDERSON CHRISTISON. Chicago, 1899. Pp. 140. \$1.25.

NOTES.

THE American Psychological Association is holding, at Yale University, its annual meeting as we go to press. In spite of the fact that a number of the leading members are abroad the meeting promises to be successful, about forty papers having been already offered. The discussion will be on 'The Teaching of Psychology,' and will be opened by Professors Fullerton, Jastrow, Aikens and Judd. The address of the president, Professor John Dewey, and the Proceedings will be published in the next issue of the REVIEW.

THE subject for the discussion before the American Society of Naturalists at its New Haven meeting during Christmas week is 'The position that universities should take in regard to investigation.' The American Psychological Association is represented by Professor Jastrow.

THE Provisional International Committee on a catalogue of scientific literature met in London last August and decided to exclude psychology, except in so far as it can be included under physiology, and also decided to abandon the proposed card catalogue. It seems strange that the Provisional Committee, containing no psychologist or representative from America, should have excluded psychology from the list of the sciences after it had been incorporated by two International Conferences. In any case the abandonment of the card index makes the catalogue of comparatively little interest to psychologists, we having already two excellent annual bibliographies.

MISS LILLIE J. MARTIN has been appointed assistant professor of psychology at Stanford University during the absence abroad of Professor Angell.

